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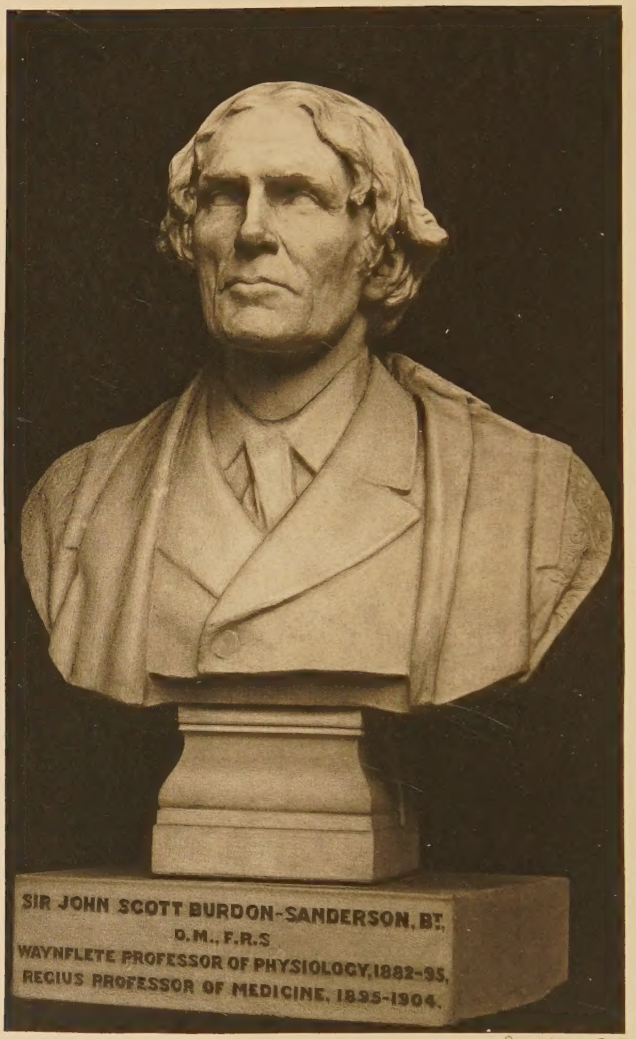
SIR JOHN SCOTT BURDON-SANDERSON, BT.
D.M., F.R.S
WAYNFLETE PROFESSOR OF PHYSIOLOGY, 1882-95,
REGIUS PROFESSOR OF MEDICINE, 1895-1904.

Emery Walker Ph. sc.

From the bust by H. R. Hope-Pinker in the University Museum

CAT.

SIR JOHN BURDON SANDERSON

A MEMOIR

BY THE LATE

LADY BURDON SANDERSON

COMPLETED AND EDITED BY HIS NEPHEW AND NIECE

WITH A SELECTION FROM
HIS PAPERS AND ADDRESSES

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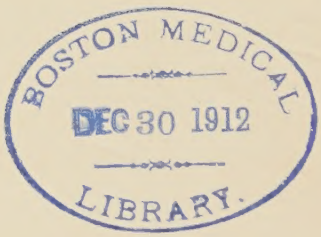

PREFACE

THE following Memoir was begun by the late Lady Burdon Sanderson. When she died, the first four chapters were approximately complete, and are printed as she left them, with a few omissions and corrections. It has fallen to us to finish the work she began; and we have added some of Burdon Sanderson's papers and addresses on subjects of general interest. We beg to offer our thanks to many friends who have rendered assistance, and more especially to Sir Lauder Brunton, Sir William Turner, Professor Schäfer, Professor Gotch, Dr. James Ritchie, Professor Vines, Sir Victor Horsley, Dr. C. J. Martin, and Miss Florence Buchanan. We are also indebted to Miss Acland for allowing us to reproduce a photograph taken by herself.

J. S. HALDANE.
E. S. HALDANE.

May, 1911

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I. MEMOIR

CHAPTER I

PARENTAGE AND BOYHOOD

1828—1847

JOHN SCOTT BURDON SANDERSON was born at Jesmond, near Newcastle-on-Tyne, on December 21, 1829. This busy town was at that time two miles distant from the 'township of Jesmond', which was still in the midst of open country and outside the boundary of the borough and county of Newcastle, though a few years later included within it. It retained its rural character until the last third of the nineteenth century, when much of the land was sold, roads were made, and houses built, and Jesmond became for the most part a suburb of a populous city. Happily, the most beautiful portion of Jesmond Vale, or Dene, has been preserved by the generosity of the late Lord Armstrong, who turned it into a pleasure-ground, which he presented to the Corporation for the use of the public.

Tradition has it that the Burdons, or Bourdons, came into England with William the Conqueror; and a palmer's staff in the family coat-of-arms is said to indicate that some of them took part in the Crusades. The name is not found, however, in any Northumbrian documents until 1486, when Thomas Burdon was Mayor of Stockton. Of John Burdon Sanderson's great grandfather, Mr. Richard Burdon, the only facts which have been recorded are that he was fond of all country pursuits, and lived to a hale old age, riding across country to the last year of his life with the fearlessness of youth, and much of youth's freshness and vigour. He owned the small property of Brunton, and his residence in the Shieldfield is remembered by one of his great grandchildren as a long, low house, with a grassy common in front of it, from which it was not separated by any enclosure. It was supposed to have been the house in

which Charles the First, when he was practically a prisoner in Newcastle, was accustomed to rest when playing golf on the neighbouring course. Nothing now survives of the old Shieldfield but a little triangular patch of grass; the whole district having been built over. Richard Burdon's second son, Thomas, was born at Shieldfield in 1758, and in 1786 was married to Jane Scott, the daughter of John Scott, a Hoastman[1] of Newcastle, and the sister of the distinguished lawyers, John and William Scott, who are better known by the titles which they afterwards bore of Lord Eldon and Lord Stowell. She is said to have been a woman of a remarkably fine presence, and not much inferior in mental power to her celebrated brothers.

Thomas Burdon, besides carrying on successfully the business of a brewer, took an active part in some of the stirring events of his time. One of these has been thus narrated:

'The sudden reduction of the navy consequent on the victory of Waterloo, along with the arrival in the northern ports of the local fleet of Greenland whalers in the autumn of 1815 for their winter quarters, threw out of employment large numbers of seamen. These demanded that every vessel should employ five men and one boy for every hundred tons register, and on the refusal of these demands they prevented the sailing of ships from Blyth, Shields, and Sunderland.'

The methods of these strikers were not quite the same as those employed in the present day, but were probably not less effective. It is narrated that they 'forcibly prevented the sailing of all ships, by taking out the seamen, whom

[1] 'A Hoastman was a member of a corporation or merchant guild in Newcastle-on-Tyne who had originally the function of receiving strangers (called Hosts or Oasts) who came to buy coal and certain other commodities, and of conducting their purchases, on which they levied a certain duty; in later time they controlled the selling and exportation of coal.'—*The Oxford English Dictionary*, edited by Sir James Murray. As an illustration a quotation is given, which says 'Jack Scott, the Newcastle Hoastman's son, who ran away with Bessy Surtees, and who was afterwards known as Lord Eldon'.

they compelled to join their body, under pain of having their faces blacked and jackets turned, and being thus exhibited through the public streets, with other contemptuous treatment.'

Seven warships were sent to Shields, with troops of infantry and cavalry to support them. On the 21st of October the magistrates of Newcastle and adjoining localities (probably Shields), accompanied by local volunteers, succeeded in overcoming the riotous sailors and liberating the detained ships, so that within a few days 150 vessels, many of which had been in port for several weeks, were enabled to set sail. This was accomplished without loss of life or serious injury to any one. In acknowledgement of Mr. Burdon's zeal on this occasion, as well as of the interest he had taken for many years in the volunteer movement, the honour of knighthood was conferred upon him by the Prince Regent in May, 1816.[1] He was a Lieutenant-Colonel of the South Tyne Hussars, and took a special interest in the Parliamentary elections of 1826, when he actively supported the Conservative candidate, Mr. Liddell, being chairman of his committee. He caught cold in the course of the elections, and, after recording his vote on July 20, he became seriously ill and died six days later.

Sir Thomas left three sons, the third of whom was Richard, the father of the subject of the present memoir. He was born at the Manor House of Jesmond in 1791. At the age of six he was sent to school at Ovingham-on-Tyne. What instruction he received in the rudiments of learning is not recorded, but it is stated that he was taught to swim by a rough-and-ready method. The smaller boys were held by one leg and thrown into the river, and scrambled out as

[1] See *Men of Mark 'twixt Tyne and Tweed*, by Richard Welford, and a book published in 1883, entitled 'Local Records, or Historical Register of remarkable events which have occurred in Northumberland and Durham, Newcastle-on-Tyne and Berwick-upon-Tweed, from the earliest periods of authentic record to the present time,' by John Sykes.

best they could. In a somewhat similar manner, as he long after related to his own children, he had been taught to ride by his grandfather. The old man used to take him out to ride with him, mounting him on a favourite mule, which was, however, by no means always inclined to follow the horse over fences, and sometimes resolutely stood still. On such occasions the boy's grandfather rode on quietly telling him to 'come along', assuming that he would have no fear. This assumption was fully justified, for even till old age Mr. Richard Burdon was a fearless horseman and a good whip.

When twelve years old he was transferred to the Grammar School at Durham, a school which was of good repute as regards education.[1] The discipline, however, was very severe, a circumstance which may in later years have influenced his decision not to send his sons to a public school. From Durham he went for a year to read with the Rector of Edlingham, the Rev. Dr. Manisty, who became connected with the family by marriage, and who was the father of the late Mr. Justice Manisty.

In 1808 he was admitted to Oriel College, Oxford. He gained the Newdigate prize for a poem on the Parthenon, and obtained a first class in Classics in 1812. In 1813 he was elected Fellow of his College, and in 1814 received the Chancellor's Prize for an English Essay, the subject of which was 'A comparative Estimate of the English Literature of the Seventeenth and Eighteenth Centuries'. The Encaenia of this year was graced by the presence of the allied sovereigns. Prize poems specially written for the occasion were recited before the distinguished visitors, but the usual prose recitations were omitted, to the disap-

[1] It may be doubted whether this reputation was altogether deserved if an anecdote related of the master, whose name was James Britton, were true. He used to ask the boys who was the best classical scholar in Oxford, and without waiting for a reply solemnly added, 'James Britton, boys, James Britton'. Another school tradition tells of his indignation at the request of a stranger who visited the school that he might be permitted to hear the boys sing. 'Zing, Sir', he answered, 'we don't zing here. This, Sir, is the Grammar School!'

pointment of the young prizemen who had hoped for the honour of taking part in them.[1]

Of contemporaries with whom he was in intimate relation he spoke in after years as belonging to a group of young men who 'sharpened each other as iron sharpeneth iron'. Four of these—Whately, Attfield, Keble, and J. T. Coleridge—had gained in succession the prize for the English Essay in the years immediately preceding that in which the same honour came to Burdon. Two of these friends, Whately and Keble, were his companions in a vacation tour in the Isle of Wight. To the teaching of Archbishop Whately (as he afterwards became) Burdon used to say he was much indebted, and he ever retained a grateful remembrance of his former friend, although in later years the difference in their careers, and the divergence of their religious opinions, prevented any intimate intercourse.

With the mental powers which he undoubtedly possessed it may well be believed that he would in time have made his mark in the world, but before he left Oxford a radical change took place in his opinions, which completely diverted the current of his thoughts, and banished from them all worldly ambitions. He was always strict in his religious observances, and in the early period of his College life he attended Chapel morning and evening, and fasted twice a week. Somewhat later he came into intimate relation with Mr. Brandram, a member of Oriel, who graduated a year later. Through the influence of this gentleman, who was afterwards Secretary of the Bible Society, Burdon was gradually weaned from what would now be called High Church tendencies, and joined a company of young men at St. Edmund Hall, who were

[1] The writer is indebted to Mr. Leudesdorf, Registrar of the University of Oxford, for the following information: 'At the Encaenia in 1814 the sovereigns present were—the Emperor Alexander I of Russia, the King (Frederick William III) of Prussia, and the Prince Regent (afterwards George IV). There were several other members of royal houses present—three or four princes of Prussia, the Princess Royal of Würtemberg, Prince Charles of Mecklenburg-Strelitz, the Prince of Orange, &c.'

designated by those who did not share their views 'a nest of Methodists', the leader of this little company being Mr. Daniel Wilson, afterwards Bishop of Calcutta. Burdon's intercourse with his new friends led to a revolution in his ideas. 'All was changed to him', as he said later to his wife; 'he was like another man, and the renunciation of worldly ambition was nothing compared to the peace of mind which he had gained.' Other circumstances, to be immediately referred to, probably had their share in influencing his life.

He left Oxford in 1814, and in the following year was married to Elizabeth Skinner Sanderson, the only surviving child of Sir James Sanderson, Bart., banker and hop-merchant of the City of London, in accordance with whose will he took the name of Sanderson. This gentleman was well known in the City, where he had held the office of Sheriff, and twice that of Lord Mayor; his first mayoralty in 1792 coinciding with 'a time when the principles of the French Revolution were contaminating the minds of men', opinions which 'required to be counteracted by a firm, prudent, and constitutional chief magistrate'. So admirably were these conditions fulfilled that 'his sovereign acknowledged them by creating him a baronet'. The late General Fisher, one of his nephews, has recorded that Sir James was a great favourite both with the King and with Mr. Pitt. He was an accomplished, elegant, and gentlemanly man—proud, arbitrary, and independent. He was member of Parliament for Malmesbury in 1792 and for Hastings in 1796, and his portrait, that of a very handsome man, may be seen in the Court Room of Bridewell Royal Hospital, of which he was a Governor. He was a stanch supporter of Pitt, whom he thought one of the greatest men who had ever lived.

Sir James died in 1799. His widow, with her only child, who was two years old at the time of her father's death, went to live at Clapham, and became intimately acquainted with members of what was called the 'Clapham Sect'.

When old enough her child was sent to a school at Balham, which was attended by children belonging to these families. It was a dame's school, attended by both boys and girls, and one of her schoolfellows was Macaulay, afterwards the distinguished historian.

Having been entered at the Middle Temple in 1812, Mr. Burdon Sanderson was given the Secretaryship of Presentations by Lord Chancellor Eldon in the following year, and in 1814 he was made a Commissioner in Bankruptcy. While holding these offices he frequently accompanied his chief to the House of Lords, and at times shared in the unpopularity of the Lord Chancellor. On one such occasion it is recorded that during the Burdett riots a stray shot fired by one of the gathering mob, and intended for the Lord Chancellor, passed through the skirt of the coat of his young companion. As Secretary of Presentations he soon found that the bestowal of preferment was governed far more by political than by religious considerations. Indeed, the candidates for livings understood this so well that they frequently founded their claims on services rendered to the political party then in power. For example, one clergyman pleaded that he had raised a troop of Yeomanry, another that he had voted for Lord Eldon at Oxford, while a third had the effrontery to offer a ten per cent. commission on the value of any living to which he might be appointed by the Chancellor. 'I well remember', his nephew said long afterwards, 'the indignant scorn with which that high-minded nobleman showed me the letter.' Religious convictions led Mr. Burdon Sanderson to regard these appointments as a 'traffic in souls', and after holding the office of Secretary for about eighteen months he resigned it, 'solely', as he said, 'for conscience sake'. When he resigned the Secretaryship he also gave up the Commissionership in Bankruptcy, a decision which in after-life he regretted. This proceeding naturally vexed and disappointed Lord Eldon, who had started his nephew on a career which he had reason to expect would have been a

successful if not a brilliant one; and caused an estrangement in their relations to each other which lasted some years. Mr. Burdon Sanderson long afterwards said, as regards his own feelings towards his uncle, 'that he both respected and loved him.' Eventually a reconciliation between them took place. In a letter written at a later period Mr. Burdon Sanderson gave a charming little sketch of the aged Chancellor as he saw him on his last visit to his Durham property. 'It would have made a picture,' he wrote, 'as the old courtier stood one day before the door of one of his tenants—a venerable old man—holding his hat in his hand, and positively refusing to put it on until the old man should also cover his own white head.' Shortly after his marriage Mr. Burdon Sanderson took up his abode near Tunbridge Wells, where he lived until, on the death of his father in 1826, he was recalled to Northumberland. For several years he led the life of a country gentleman, and was a lover of sport, both hunting and shooting. The conviction at length forced itself upon him that this was a useless and selfish life, and that he ought to devote himself to the welfare, material and spiritual, of his fellow men. His thoughts had probably been gradually tending in this direction, and they may have been strengthened by a visit he had about that time from the Hon. Paul Methuen, who had joined the Plymouth Brethren, at that time just beginning to make themselves known. Mr. Methuen had gone to the north to try and persuade Mr. Burdon Sanderson to attend a Conference of that body which was shortly to be held in the south of England. On seeing his host his first words were, 'As I approached your house I heard the unholy barking of dogs!' This greeting was often afterwards referred to with amusement. Nevertheless, it may be conjectured that the conversations with Mr. Methuen which ensued may have had some influence on the decision which seemed so sudden. Mr. Burdon Sanderson sold his hounds and horses, relinquished society, and eventually bought a chapel in Newcastle-on-

Tyne, where he held frequent services. Meanwhile, having made some study of architecture, he had built himself the house at West Jesmond which was his home for many years. The grounds around the house were tastefully laid out; they were bounded on the west by the town moor, and on the other sides by the properties of well-known citizens of Newcastle. Mr. Burdon Sanderson must in his youth have been very handsome. When the writer became acquainted with him his hair was quite white, although he was not far in advance of middle life; but his face showed few traces of age, his personality was most attractive, and he had the charming manner and dignified courtliness of an earlier generation.

Of the five children of Mr. and Mrs. Burdon Sanderson the subject of this memoir was the second son and fourth child. He was born before the new house was completed, and here, or at a small house on the moors near Otterburn, his childhood and youth were mainly passed. Among his earliest recollections were those connected with Biddleston, where the family in 1832–3 resided for several months during the building of the house at Jesmond. The house is described in a letter written by his father as having been beautifully situated 'in one of the most retired parts of the country, on a commanding eminence—about half-way up the hill. Immediately behind, there runs a deep ravine tastefully planted, in a great measure by the hands of nature . . . beneath which a brook runs murmuring over its pebbly bed. In the remote distance rise the dim forms of the Cheviot range, where masses of cloud lie brooding, as if waiting the command of the sun to disperse and unfold the full beauty of the landscape.'[1]

John Burdon Sanderson's childish impressions of Biddleston remained vivid to the end of his life, and he once revisited it with the keenest interest and pleasure.

[1] Biddlestone was regarded as the Osbaldistone of Sir Walter Scott, and the description just given is sufficiently like that in *Rob Roy* to justify this supposition.

His sister, Mrs. Haldane, has furnished an account of Otterburn, and some notes of her earliest recollections of her brother John:

'Otterburn Dene house, and the land on which it stood, had been the property of Sir Thomas Burdon. It looks across a valley which he had planted, and is surrounded with undulating hills mostly covered with heather. The house was originally quite small, and occupied by the gamekeeper and his wife, two rooms being reserved for Sir Thomas when he came there for shooting. It was only approached by a cart-track. The old coachman, who was in the service of the family for thirty-five years, managed, however, to drive a carriage and four ponies for two miles over the moor from one of the main coach-roads. Our father added to the house so as to make it habitable for the family, and changed the name from Davy Shiel to Otterburn Dene. There were numerous stone cists, containing bones, to be found scattered about the moor; there were also tumuli, as well as other indications of skirmishes at various periods, and peel towers were also to be seen. The battle of Otterburn was fought at the edge of the plantation, and a cross was erected at the spot where Percy is supposed to have fallen. At Elsdon, a short distance away, a place is pointed out as the Mote, where popular meetings were held to discuss matters of local interest.

'I recollect my brother as an infant, being nearly four years his senior. He was a beautiful child, very fair with bright auburn hair and large deep blue eyes. He was very observant and quick in understanding. I taught him to spell a few years later and knew what his powers of learning were.'

He was at that time quick tempered, and his sister suggests that this may have arisen from the fact that he was so adored by the whole family that every one gave way to him. This may in his childhood have been the cause, but even in after-life there were occasional bursts of displeasure. These were almost invariably called forth when he observed idleness or carelessness in work, and not by any personal offence, and they were scarcely more than momentary. He had such an avidity for work himself that he was annoyed at any appearance of slackness or want of interest in others. When John was a young boy

his father had a huntsman named Robinson, who looked after the hounds and had some supervision of the horses:

'My brother John and he were great friends,' his sister says, 'and the former enjoyed hearing all the anecdotes about the dogs and horses, and he was attracted also by the fearlessness of the huntsman. Robinson knew a great deal about birds, their notes and habits, as well as the best waters for fishing. Both these circumstances had their effect upon the child. Fishing was the sport which chiefly attracted him, and the observation of birds was one of his great pleasures. This interest was stimulated by a friend and neighbour of the family, Mr. Bertram Mitford, who imparted much information to the boy, not only about birds, but as to other facts of natural history, which there was abundant opportunity of studying on the moors about Otterburn. When he was about six years old he was fond of reading Stanley's book on birds, Captain Cook's *Voyages*, and other works relating to travel or to natural history. His fondness for fishing was stimulated by long fishing excursions with one of his tutors, a Mr. Webster, who was in full sympathy with a boy's pursuits and who did not mind wading through streams or rivers, however rough the weather might be.'

'My brother John,' Mrs. Haldane continues, 'had a very good voice, correct and musical. He and my eldest sister often sang together. He also played the violin, and I often accompanied him on the piano in works by Mozart, Haydn, or Dussek. Woe betide me if I played a false note; his quick ear detected it immediately, and he and his violin nearly jumped off the ground.'

The violin, unfortunately, was given up when he became absorbed in scientific studies in the University of Edinburgh, but he continued singing for several years. He had a good baritone voice, but its quality was seriously impaired by an illness from which he suffered in 1858. He gave up solos, but continued choral singing for some time longer. His intense love of music never deserted him. During his last illness it was a pleasure and relief when kind friends would spare a little time to play to him.

One of Burdon Sanderson's pleasures as a lad was riding to the post office in Newcastle to get the letters, as by this

means they were received earlier at Jesmond than they would have been by the ordinary delivery. He much enjoyed these rides, although he was never a very good horseman. He was a dreamy lad, and probably was often thinking of other things than those immediately present to him. On one occasion, when he was about fourteen, he was thrown by his pony, which galloped home riderless, to the great alarm of his mother. When in due time he sauntered in unhurt he was soundly rated by his father for being so stupid as to be thrown.

As has been mentioned already, Mr. Burdon Sanderson, while his children were still young, retired from society and gave up his ordinary occupations in order that he might devote himself to a religious life. This course of action necessarily influenced the whole family, and centred their thoughts while still children, as his son John afterwards believed, too exclusively on their own feelings in relation to God and a future life. He became convinced that his religious emotions had been too much stimulated, that he lived as regards his moral and religious feelings in what might be called a hot-house atmosphere. Thus, as it seemed to him at a later period, he had been led to exaggerate unconsciously, but quite honestly, the intensity of his feelings. The following quotation from a letter written to him by his father just after he had passed his tenth birthday is given in illustration of what has just been said:

'I am glad the Lord speaks so graciously out of His Word, telling you no more to consider yourself a child; and I hope you remember the apostolic commentary on that subject, "Nevertheless, in malice be you children, but in understanding be men."'

After recommending the child 'not to be lifted up' the letter continues:

'I pray God to keep you humble and teachable, that you may not be exalted with the abundance of the revelations, nor yet shut your ears to the voice of His Spirit.'

The absolute sincerity and truthfulness of the parents commanded the respect of their children, and this, combined with the affection existing between parent and child, prevented the evil results which might have been expected to occur to the moral character of the children when, in time, the fervour of their childish faith was cooled.

Meanwhile the boy's secular education was not neglected. He was taught partly by his father and partly by the tutors of his elder brother. His father, being himself a good classic, was quite competent to give the required instruction, but his impulsive nature rendered the lessons trying both for teacher and pupil. The son never allowed that he himself possessed more than a very moderate knowledge of Greek, but in consequence of his father's extreme impatience when he made a false quantity his ear was always sensitive to a fault of this nature.

The happiest time of the year for all the children was undoubtedly that which they spent at their Otterburn home. The boy delighted in wandering at will on the moors, and in fishing in the trout streams. It is not known that he was a collector, at any rate no collections have survived, but he was always observing the plant and animal life around him, and he treasured up in his mind many facts which were useful to him in after-life, besides cultivating the habit of observation so essential in the pursuit of science. His intimate acquaintance with the Otterburn moors was shown when after a long absence he revisited them with his wife.

He said, as they were rambling together, 'Somewhere hereabouts there used to be a plant of white heather.' After a few minutes' search he came upon the plant, bearing its white flowers, whereas in the course of a long walk no others but those of the ordinary colour were found.

Excepting a short visit to London when he was a lad, and an excursion to Scotland when he was just grown up, his life was spent either at Jesmond or among the scenes

just described, until at the age of nineteen he went to the University of Edinburgh. It would have seemed natural that Mr. Burdon Sanderson should have sent his sons to his own University of Oxford, but in a time when tests still existed, it would not have been consonant with his feelings to allow them to submit themselves to such engagements. An English University education was therefore denied to them as to many others of their contemporaries. It is worthy of note that Burdon Sanderson and Michael Foster, the two most prominent physiologists of their time in England, were both excluded by tests from education in the Universities where they afterwards became famous.

CHAPTER II

YOUTH AND EDUCATION

1847—1852

Burdon Sanderson's father had intended that his son should enter upon the study of law, and although he had long ago relinquished all ambition for himself, he may have entertained in the background of his thoughts a hope that the young man whom he had named John Scott after his illustrious relative might achieve some success in the profession in which his great-uncle had attained such distinction. All the son's inclinations, however, tended towards natural science, and his intercourse with Dr. Mortimer Glover directed his thoughts to medicine. Burdon Sanderson's only brother was laid up at Otterburn for many weeks with a dangerous illness, and the professional aid of Dr. Glover, then a practitioner in Newcastle-on-Tyne, was secured to watch over the patient, whose protracted illness necessitated a long sojourn of the medical attendant. Dr. Glover's interests, like those of other eminent practitioners, were not bounded by his services to the suffering. The sciences kindred to medicine attracted him. He was the author of a Manual of Chemistry and wrote on Pathology and other allied subjects. During his prolonged stay at Otterburn many opportunities offered themselves for intercourse between Dr. Glover and the young man, whose thoughts were then concentrated on the important question as to the course of his future life. The profession of law had no attraction for him, and there is little doubt that Dr. Glover's personality and the conversations they had during their many wanderings together on the moors influenced Burdon Sanderson and led him to his choice of the profession of medicine. The opportunities

for observation and research in the biological studies which precede those of practical medicine were especially attractive to one whose inquiring mind even as a child had led him to investigate the varied aspects of Nature which presented themselves to him in his daily life. He said in later years that he remembered the exact spot where he stood pondering the question of his future, and where he finally made his decision. His father at length yielded to his wishes, and he shortly after entered upon that study the prosecution of which was continued as long as his strength permitted, and his interest in which never flagged to the end of his life.

As his place of study Edinburgh was selected, in the University of which was a flourishing School of Medicine, renowned then as perhaps the best in the kingdom. There he matriculated on the 2nd of November, 1847, in the name of John Scott Sanderson. He did not until a few years later habitually use his father's name of Burdon.[1]

One of the earliest courses of study which he had attended was that of Botany, a subject which had from his childhood interested him greatly. The weekly excursions conducted by Professor Balfour were always a source of pleasure to him, and his delight was enhanced when he found a plant unknown to him or one of rare occurrence. In one of the few letters written during his undergraduate life which has been preserved he speaks of a delightful walk 'along the canal as far as Currie, where there are beautiful woods which we traversed, and then proceeded to Ravelrig bog, where we found the little *Listera cordata*, *Sedum villosum*, and a rare plant *Corallorhiza innata*, besides Sundew and other common bog-plants.'[2]

[1] As a child he was always called by his two names, John Scott. When he settled in London as a young medical practitioner the circumstance that another young doctor of the same name took up his abode in a neighbouring street, and that letters and messages frequently went to the wrong Sanderson, led to the constant use of his patronymic, and finally he registered his arms in the name of Burdon Sanderson.

[2] Sir Joseph Hooker, who has kindly interpreted the names of

In another letter he refers to the summer classes as a pleasant change from his ordinary occupations, 'although', he adds, 'I cannot well be engaged more agreeably than I am at present.' Professor Goodsir recommended him to rise at five in the summer, to dissect from six to eight, and then go to botany, and afterwards spend the day as he liked. He doubted whether he would have resolution enough to follow this advice. It is not known whether he tried the plan, for he seems to have already acquired the habit of working far into the night, a practice which he continued for many years. Eventually, however, and even late in life, he was an extremely early riser.

The teachers with whom he was in the closest relation, and who chiefly influenced him in his student days, were Professors Balfour, Goodsir, and Bennett. The study of botany necessarily brought him into relation with the first of these professors, with whom he formed an intimate friendship; for Professor Goodsir, who was a remarkably successful teacher of anatomy, he had the utmost respect; and the fact that the lecturer did not satisfy himself with giving a mere descriptive account of the various structures he was called on to expound, but pointed out the relations of his science to physiology, pathology, histology, etc., greatly enhanced the value and interest of his teaching to one, at any rate, of his pupils.[1]

Professor Hughes Bennett was elected to the Chair of the

these plants, which were indistinctly written, stated in his letter that the last-named 'is a very rare British plant, now I am told extinct in Ravelrig bog.'

[1] The following incident illustrates the personal influence exercised by Professor Goodsir. In 1848, just after the French revolution, a spirit of unrest took possession of the Edinburgh students, and they 'got up a row' which led to the arrest of several of their number. This action still further inflamed them, and they tried a few days later (as a letter written at the time states) 'to get up another row'. The police on this occasion were in plain clothes, scarcely daring to show their faces, and almost all the students were armed with cudgels. Professor Goodsir did not attempt to give his lecture, but the influence he possessed enabled him to persuade the rioters to go home peaceably by four in the afternoon.

Institutes of Medicine in 1848, and although he had been a teacher in Edinburgh for some years it is improbable that Burdon Sanderson had come into relation with him before his election to the professorship. In some manuscript notes for an address delivered in Edinburgh in 1901 on the occasion of the opening of a new laboratory given to the Physiological Department of the University as a memorial to Professor Bennett by his daughter Mrs. Cox, Burdon Sanderson says:

'If I were asked in what way Bennett contributed most to the advancement of medical education I should without hesitation answer that his most important achievement was the introduction into the Edinburgh School of an exact method of studying the characteristic phenomena of disease . . . whether by the unaided senses or with the aid of instruments. Of the instruments then available the microscope was the most important. In 1841 that instrument, now so familiar to every student, had not found its way either into the ward or into the physician's consulting room.'

Professor Bennett, immediately after his appointment, announced that his lectures would be accompanied by microscopical demonstrations, 'for which twelve achromatic microscopes of great power, by Chevalier of Paris, would be used'.[1] He adds that 'his teaching as Professor was on the same lines with that which he had given for so many years outside of the University, his guiding principles being, firstly, not to advance any pathological doctrine which could not be supported by anatomical or physiological data, and, secondly, to keep constantly in mind that the men he had to teach were being trained to be doctors.' At the same time that Bennett became Professor in the University, he was made Professor of Clinical Medicine at the Infirmary, and was therefore able to carry out the method of clinical teaching which he had pre-

[1] 'Other teachers, notably Professor Sharpey at University College, London, had exhibited microscopical preparations in illustration of their lectures, but Dr. Bennett was the first to place the microscope in the students' hands.'

viously elaborated at the Dispensary. 'He was thus enabled to blend together physiology, pathology, and practice', as he himself expressed it, 'into one glorious hotch-potch!' Burdon Sanderson proceeds: 'You will not misunderstand me if I add that, judged from the point of view of the physiologist of to-day, the instruction given was incomplete, for it comprised only those chapters which are of special interest to the physician. No such mixing of subjects as I have quoted would in the present state of science be possible.' Those who are aware of the large place which both physiology and pathology occupied in Burdon Sanderson's later studies, and of his belief in the intimate connexion of the biological sciences with each other, with the physical sciences, and with medicine, will be able to appreciate the influence which the teaching of Bennett and Goodsir must have had upon him.

Sir William Turner, Principal of the University of Edinburgh, who has kindly contributed some notes concerning the period of Burdon Sanderson's student-life, states with regard to the Royal Medical Society of Edinburgh—which has long held, and still holds, an important position in the School of Medicine—that 'the best students join it. One duty they have to discharge is to prepare, read, and defend a dissertation on some medical or scientific subject.' On the 17th of November, 1848, this duty was performed by Burdon Sanderson, the subject being Vegetable Irritability. 'It is interesting to note', Sir William says, 'that even as a student his attention had been directed to the irritability of plants, a subject which he so carefully investigated in later life.' In the session of 1850–1 he was chosen by his fellows to be the third President of the Society, there being three in each year. This was before he obtained his degree, and 'shows the high estimation in which he was held by his fellow students'. His own view of the appointment was, however, different. In a letter written at the time to a member of the Society he stated that the idea of his being elected had appeared to him 'chimerical', that

he thought the voting 'disgraceful', and wished to know why he was placed 'above Cuningham'. This little incident illustrates his innate modesty, and a tendency, which continued throughout his life, to under-estimate his own scientific attainments. The student referred to remained his friend until death parted them. Dr. Cuningham became eventually Surgeon-General and Sanitary Adviser to the Government of India.

Among Burdon Sanderson's papers was found a copy of his Address to the Medical Society on his election to the Presidentship. A few extracts may not be without interest, as showing how, while still an undergraduate, his thoughts were occupied with the subject of the scientific groundwork of Medicine. After expressing thanks for his election, and a few other prefatory remarks, he refers to regrets which had been expressed on account of the diminution of 'enthusiasm' shown in the discussions of the Society. He thinks it was not so much a subject of regret because, in his view, it was an indication of the important change which had taken place in the science of Medicine.

'At the period of the origin of this Society', he says, 'a system of Medicine founded entirely on speculation reigned paramount, under the auspices of the then Professors of the University, who were the pupils of the illustrious Boerhaave. Although these doctrines were received with implicit credence in every other quarter in this country, those were to be found (in Edinburgh) who not only entertained but expressed their doubts of their consistency with truth. Haller, a Swiss savant, shortly after for the first time applied the inductive method of reasoning to the science of Physiology, of which he has been justly styled the founder. His doctrines, though elsewhere disregarded, found supporters among the members of our Society, and between them and those who still clung tenaciously to the speculations of the Leyden School, controversy arose which was subsequently carried on within these very walls, and which was fraught with the most important results. At that time Cullen was a member of this Society, and there can be no doubt that he took an

active part in this discussion, as we find that after his elevation to the professorial Chair in the University, his first act was to substitute for the vague speculations of his predecessors those imperishable truths which have rendered the reputation of their advocate as imperishable as themselves. Thus the career of this great man, which originated with the origin of this Society, only terminated in his giving Medicine a name for the first time among the sciences, and in placing our University in a position which for a long series of years it occupied—that of the first Medical School in Europe.'

He then refers to a still later time, when another great controversy agitated the world, that on the so-called Brunonian theory :

'The opinions of Brown were discussed in this Hall with a warmth and animation of which, I suppose, we cannot form any adequate conception, and though long and ardently supported by those to whom indolence or an ungoverned imagination were an inducement to prefer speculation to truth, tended rather to increase than to weaken the prevalence of the Cullenian doctrines. . . . Our knowledge is now laid on a foundation more certain, more enduring than was theirs. We are engaged, as many of us as are studying our profession in the right spirit, in endeavouring to multiply our knowledge of actual phenomena by accurate observation. . . .How many of the questions which were discussed by our predecessors less than twenty years ago have been, by the accumulation of ascertained facts, now set at rest.'

The centenary of the Medical Society was an epoch not merely in its own history but also in that of medical science, for it was the year [1838] in which Schleiden's discovery was given to the world.

'Since that period how many other important facts have been enunciated. Need I allude to the researches on the physiology of the nervous system of two of our Presidents—one the early termination of whose career our science, and we especially as members of this Society, have deeply to regret[1]. The other, Dr. M. Barry, may be considered as one

[1] The reference is doubtless to John Reid.

of the founders of a branch of inquiry which has been pursued with a success which is almost incredible by German observers.'... 'The time is past... for mere speculations, however ingenious.... No second Boerhaave can now arise to lay down a new system which by the attractiveness of its dogmas shall cause us (forsaking those results which we arrived at by patient and laborious investigation) to fall down and worship it.... We must direct our whole energies in earnestly striving, not against each other, but against those barriers which Nature is ever placing in the way of those who would penetrate into her mysteries; remembering that the one only object which we must all have in view in every attempt which we make for the promotion of the interests of our science, is the Discovery of Truth.'

In the Address at the opening of the Bennett Laboratory, already referred to, Burdon Sanderson quoted a remark of Professor Bennett, to the effect that he himself, as well as many other students entered on their medical curriculum with considerable familiarity with the external aspects of disease but with no theoretical training. This was a consequence of the custom then prevalent of apprenticing a would-be Doctor of Medicine to one already in good practice before he entered upon his studies. Such men as these were brought in the Edinburgh Medical School into relation with another class of students younger than themselves, who had seen no practice but had a certain amount—rather less than more—of instruction in physiological science; and again with others who had received a University education. It was the mixture of these different elements that gave an Edinburgh medical education its exceptional value, for there was no other school of which it could be said that there was that intercourse of men trained in different ways which was in itself an education. Out of these varied elements many friendships were formed, and thus (he adds in the Address) it 'came about that in Edinburgh at that time love and learning were linked together, according to the well-known line in which Tennyson describes the joy which arises when "one who loves and knows not, learns from one that loves and knows".' This, surely, was characteristic

of himself throughout life. He was always ready to learn not only from his seniors and contemporaries, but as willingly from those of a younger generation.

In 1851 Burdon Sanderson received the degree of Doctor of Medicine, having presented as his graduation thesis a paper entitled 'On the Metamorphosis of the coloured Blood Corpuscles', for which he was awarded one of the graduation gold medals. The dissertation consists of an extensive criticism of the subject, founded on numerous experimental observations made by him upon the blood of the spleen and other organs. The other two gold medallists of the year were his friends to the close of their lives; both were widely known in medical and scientific circles. Charles Murchison became a medical practitioner whose clinical lectures at St. Thomas's Hospital attracted large numbers of students, and who was at that time a chief authority on typhoid fever. His death at a comparatively early age was much deplored. The other, Dr. Spencer Cobbold, was known for his scientific researches, and especially for his work on the Entozoa.

Some reminiscences of Burdon Sanderson's early life, supplied by Sir Alexander Christison and Dr. David Christison, his contemporaries in the University and sons of the celebrated Professor of the same name, mention the expectation of his fellow students that he would make his mark in the years to follow, and rise to distinction in his profession. He was always listened to with interest in the discussions at the Royal Medical Society, and he was one whom 'the most frivolous of his fellow students could not help admiring and respecting'. In person he is said to have been tall, erect, and dignified, with an intellectual countenance, 'habitually grave, as if his thoughts were always fixed on his studies, but readily relaxing into a pleasant smile when he entered into conversation.'

Before passing to the next stage in his life it may not be without interest to refer to Mrs. Haldane's notes in which

she describes two journeys she and her sister made with their brother during the vacation in 1849. The head-quarters of the family during that summer and autumn were near Rothesay in Bute, whence excursions were made in the island in all directions. 'My brother', Mrs. Haldane says, 'was keenly interested in botanizing. I remember him so well sitting on the Barone Hill minutely examining the structure of leaves and stems that we had picked up.' A longer expedition was made beyond the island, the first part of which, however, was rendered less enjoyable from the stormy weather. As they were steaming along the Crinan Canal the rain was pouring down and the vessel much crowded, and therefore her brother walked. The many locks that had to be passed occasioned delays which no doubt enabled him to keep up with the steamer. Arriving on the other side of Cantyre, they transhipped into a steamer for Oban. The bad weather pursued them, wind and rain attending them all the way. Oban at that time was a very different place from the one which now greets the arriving passenger.

'There were comparatively few houses, and there was only one hotel, the Caledonian. The promontory to the south of the town was wooded and very picturesque, and the rocks toward the south-west rose straight out of the sea. John had taken the advice of a very excellent friend, a Plymouth Brother, as to certain lodgings; but a good man is not always practical. We were taken aback at the sight of a dead sheep hanging over the door, and the appearance of the interior of the house was not reassuring. However, we put up with the accommodation temporarily, and after spending a night—I cannot say of repose—we started early in the morning. My brother went for a long walk, and I remained with my sister, who was sketching. While so occupied I heard a rustle behind me, and in a few moments saw a large black bull emerging from among the branches, his head covered with brambles. I had observed a cottage near by, and calling to my sister to throw down her sketching materials we ran as hard as we could to the small abode. The occupier, speaking only Gaelic, did not understand at first what we wanted, but as soon as she

caught sight of the bull she recognized him as an old enemy, having often feared that her children might be attacked by him on their journeys to and from school. Her husband with a dog drove the bull into his compound, and we were able to return and pick up the scattered drawing materials, and then to mount to the top of the hill, where we rejoined our brother, and whence we had a splendid view of Ben Cruachan and other mountains.'

Another journey in the autumn of the same year was made to Pitlochry, Killiecrankie, and Braemar. From the last place they drove to Balmoral and walked back. When they were on a narrow road hemmed in by steep banks they met the Queen and Prince Albert, also walking, and the ladies had scarcely room to make their curtseys with their backs against the bank. 'The Queen was dressed very simply in a grey gown and a straw bonnet with a ribbon of dark blue silk crossed over it. She was almost running to keep up with the Prince. It was a pleasant sight, they seemed so happy.'

'I have many vivid recollections of my brother on this expedition,' Mrs. Haldane continues. 'His love of truth, his contempt for useless expenditure, while not grudging to spend what was necessary, his interest in the geology and mineralogy of the districts through which we passed, his delight at the sight of an eagle flying not far from us, his love of Nature's gifts and of Nature's creatures, made it a happiness to be with him.'

Soon after receiving the degree of Doctor of Medicine, Burdon Sanderson proceeded to Paris, whither he was preceded or accompanied by several of his fellow students who desired, as he did, to advance their medical studies. The advantages offered by the Collège de France and the large hospitals of Paris were much better known in England at that time than any facilities which Germany might have offered, and the means of communication were much easier. The French capital was frequented more numerously by English students than were any of the German Universities.

In the autumn of 1851 Burdon Sanderson began to work at organic chemistry with Gerhardt, but shortly after transferred himself to the laboratory of Wurtz. Here he devoted himself especially to the study of the organic compounds which are found in animal tissues.

In the beginning of 1852 he was attending lectures on embryology by Coste and on physiology by Claude Bernard. It has been difficult to procure information as to his life in Paris. A small diary for the first half of 1852, in which the daily events are noted, is almost the only available source ; of his fellow students whose names occur in it most frequently few are still living—Rutherford Haldane, Murchison, Marcet, Harley have all passed away. Two other medical students are often mentioned, Cowan and Macdonald, but their names also no longer appear in the Medical Directory. The entries in the diary show that several times a week visits were made to the hospitals of Paris—the Midi, Charité, St. Louis, Necker, Hotel-Dieu, and Enfants Malades are often referred to ; clinical lectures were attended and notes of lectures taken ; laboratory work was done daily with scarcely an exception, and a record is kept of the researches in which the writer was engaged. Little time seems to have been given to exercise or recreation ; occasionally it is recorded that he took a walk, that he played chess in the evening with a friend, or that he went to the opera. Sometimes he went to Galignani's to read the newspaper or an article in an English magazine — one on Wilkes in *The Spectator*, and one on Descartes in *The Edinburgh Review*, are mentioned as having interested him. The devotion to work which characterized him throughout his life had already taken possession of him.

Soon after settling down Burdon Sanderson joined the English Medical Society of Paris, and in due time became its President. Dr. Pavy, with whom the little diary shows him to have been in frequent intercourse, was at the same time Vice-President, and he is still amongst us and actively

pursuing the important researches which are so well known among men of science. It appears as though the Society, which had already been long in existence, met in private houses. One meeting is mentioned as having been at Cowan's and another 'in my room'. Later, the diary records that furniture was being bought for the Medical Society, and that on February 13 it met in its new room. This room was near the Luxembourg, and a small library was attached to it. It was the rendezvous of the English medical students, where they met weekly and read papers on medical subjects or communicated the history of cases observed by them in the hospitals. A discussion followed, in which all might take part. These meetings were of great value to the members, partly from the information they received as to cases of disease in the hospitals, so that they knew where to seek those which were especially interesting to them, and partly from the free interchange of opinions on the subjects discussed. Dr. Pavy, to whom the writer is indebted for these notes, believes that the Society is now extinct; and this opinion is confirmed in a letter received from Dr. Dupuy of Paris. Drs. Pavy and Burdon Sanderson met frequently in the lecture-room of Claude Bernard, who was, the former adds, a fluent speaker, who used no notes but was extremely clear in the setting forth of his subject.

There is little doubt that to the influence of this great teacher is largely due the benefit which Burdon Sanderson derived from his residence in Paris. In the diary already quoted we find that he 'went with Marcet to Bernard's, who showed them much attention'; on another day he went with Harley and Wason, and was introduced by Bernard to Magendie; and again, on March 13, he 'was with Bernard performing several experiments'. 'His veneration for Bernard', says Professor Gotch, 'lasted throughout his life, and in later years he used to say, pointing to the bust which stood above his study table, that Bernard was the most inspiring teacher, the most

profound scientific thinker, and the most remarkable experimental physiologist that he had ever known.'[1]

At the present time, when laboratories are equipped with so many instruments and elaborate appliances for experimenting, it appears wonderful that such important work should have been produced in the mean surroundings of the investigators of these bygone days.

'Nowadays the most humble College in the smallest provincial town would not accept such dens as the State offered (when it offered them any) to the great French scientists. Claude Bernard, Magendie's Curator, worked at the Collège de France in a regular cellar. Wurtz only had a lumber room in the attics of the Dupuytren museum . . . Every scientist who had no private means to draw upon, had to choose between the miserable cellars and equally miserable garrets which were all the State could offer.'[2]

These sentences refer to a period only three or four years before Burdon Sanderson was attending Claude Bernard's experimental lectures. Of these he gives a synopsis in a book of notes written at the time, and treats some subjects in much detail, notably the rôle and nature of glycogen and the phenomena of diabetes.

Although Claude Bernard's death did not occur until 1878 there seem to have been very few opportunities for the pupil to meet again his illustrious master. In 1853, in sending a testimonial which had been asked for, Burdon Sanderson received the following letter, which he carefully preserved:

'Monsieur Sanderson, je suis très vivement contrarié d'avoir tardé si longtemps à vous répondre. J'avais égaré votre lettre dans un déménagement que j'ai fait récemment et ce n'est que depuis quelques jours que j'ai eu votre adresse par Monsieur le Docteur Pavy, un de vos amis. Il m'a fait espérer que ce retard ne vous serait pas nuisible

[1] Obituary Notice by Prof. Gotch, *Proceedings of the Royal Society*, p. 3.
[2] *Life of Pasteur*, by Radot, English edition, 1906, p. 42.

à cause de votre mérite qui paraissait clairement reconnu pour obtenir la place que vous sollicitez.

Je vous envoie la lettre que vous m'aviez demandée, ne sachant pas si la chose est encore terminée. — Tout cela vous sera remis par mon ami M. le Docteur Ludovic Hirschfeld, anatomiste très distingué dont vous connaissez sans doute le bel ouvrage sur le système nerveux. Je vous prie de transmettre mes compliments à Monsieur le Docteur Marcet, si vous le voyez.

Agréez l'expression de mes sentiments affectueux.

Cl. Bernard.

Paris, 25 Juin 1853.'

The testimonial sent him on the same occasion by his other teacher, M. Wurtz, was also accompanied by a private letter, in which he says:

'Je me fais un véritable plaisir de vous adresser le petit certificat que vous me demandez et je souhaite qu'il puisse vous être de quelqu'utilité. Je vais arranger en ce moment le grand laboratoire de l'école de médecine où je compte travailler l'année prochaine. Le nôtre est en train de se désorganiser et va bientôt être transporté à la Vilette. Depuis votre départ je n'ai pas encore trouvé le temps de m'occuper de notre travail commencé; mais j'ai soigneusement conservé les capsules contenant les sels de baryte, et j'ai la ferme intention d'achever ces expériences. Adieu, mon cher Monsieur Sanderson. Saluez de ma part M. Marcet si vous le voyez, et croyez-moi votre bien dévoué

Ad. Wurtz.

Paris, 16 Mai 1853.'

The last of these letters shows that the École de Médecine, at any rate, had awakened to the necessity of providing better accommodation for its professors than the garret of which M. Pasteur spoke. Both of the letters quoted show the friendly interest taken in their pupil by these distinguished savants.

Among the attendants at the laboratory of M. Wurtz was one Jacques Papot, concerning whom various amusing incidents too trifling to record remained in Burdon Sanderson's memory. Two years after he left Paris this young

man called upon a relation of the young doctor who happened to be in Paris, to inquire as to his well-being. He brought with him a little packet which he begged might be forwarded. This he called a *croix d'honneur*, which the emperor, he said, had issued for 'the London physicians who had studied in Paris' as a memorial of the alliance between France and England. Papot added that he had always liked Dr. Sanderson and wished to send him a little remembrance, but as he was a poor fellow he could send him nothing but a pen-wiper, which was to accompany the *croix d'honneur*. This circumstance was an indication of the regard and affection borne by those who served him, not only in his early life, but until his latest days.

Burdon Sanderson was in Paris at the time of the *coup d'État* of the 2nd of December, 1851, and the terrible scenes he witnessed long dwelt in his memory. The letters which, no doubt, he wrote home, and which would have been of considerable interest, as he was an eye-witness of many of the episodes of that event, are not forthcoming, and must have perished in a great fire at the Pantechnicon Furniture Depository in London in 1874, where, after their parents' death, his sister's furniture, along with many letters and other papers, had been stored.

One little incident of the period which he afterwards related remained in his memory. A fellow student, a refugee, probably a Russian, was sauntering about the streets watching the terrible and exciting scenes, when he suddenly came to a barricade where firing was going on, and a bullet whizzed close by him. He fell to the ground, and being there thought it safest to remain perfectly quiet among the dead and wounded. When the fighting had ceased, and the dusk of evening had cleared the streets, he ventured to get up and go to his rooms. Early next morning he went to a barber's, and had himself clean shaven and his hair cut close, to the great astonishment of his friends when they encountered him. He explained

that, although he had lain as if dead for several hours, he was afraid that he might be recognized by some one who had seen him at the barricades and might be arrested on suspicion of having taken part in the fighting: so he thought it wise to disguise himself! Dr. Pavy was not in France at the time, but he remembers that not long after he more than once saw Louis Napoleon driving through the streets with officers riding close behind the carriage, each carrying a pistol pointed forwards and ready to fire.

CHAPTER III

EARLY PROFESSIONAL WORK

1852—1860

On leaving Paris Burdon Sanderson paid a visit to Bellevue, in the neighbourhood of Plymouth, whither for reasons of health his parents had migrated. While there he was informed that there was a good opening for a medical man, especially among the 'county families', and it was suggested to him that he might worthily fill it, with profit also to himself. The prospect was not, however, attractive to him, though his parents favoured the scheme because it would have kept him near to them. Eventually they yielded their wishes to his. His own desire was to settle in London, and thither he went towards the close of 1852.

On the 9th of August, 1853, he was married to Ghetal, elder daughter of the Rev. Ridley H. Herschell; and the happy life in common which then began continued for fifty-two years. The honeymoon was spent partly in Switzerland and partly in a visit to Bonn, where his wife's mother and the younger members of her family were staying. He returned to London in October, to start upon his career. Towards the close of 1853 he was appointed Medical Registrar to St. Mary's Hospital, and in the following year he was elected Lecturer on Botany, a post which he held for one year, exchanging it for a Lectureship on Medical Jurisprudence. He continued in the latter office until 1862. He held the office of Registrar till the end of 1858, and the notes in his diaries show how often he worked far into the night in preparing his records of cases.

During 1854 Burdon Sanderson was occupied in preparing an elaborate paper on 'Vegetable Reproduction', which

he wrote for Todd's *Cyclopaedia of Anatomy and Physiology* at the instance of Professor Allen Thomson of Edinburgh, who had been one of his teachers. Professor Vines, at the request of the writer, has kindly furnished in the following paragraph an estimate of Burdon Sanderson's botanical knowledge at that time, and of the value of the paper just referred to:

'Though professedly an animal physiologist and pathologist, Sir John Burdon Sanderson was something more—a biologist in the widest sense of the term: plants as well as animals were the objects of his study and research. His earliest paper, read before the Royal Medical Society in 1850 whilst he was still a student in the University of Edinburgh, was on 'Vegetable Irritability', a subject that again engaged his attention thirty years later when he entered upon his notable investigations of the electromotive properties of the leaf of *Dionaea* (1877-8). In the same year he published in the *Transactions of the Botanical Society of Edinburgh* another more purely botanical paper, on the embryogeny of *Hippuris vulgaris*. Such was his proficiency in this science that, after he had settled in London, he was appointed in 1854 Lecturer in Botany at the Medical School of St. Mary's Hospital, an appointment that was amply justified by the elaborate article on Vegetable Reproduction that he contributed about that time to Todd's *Cyclopaedia of Anatomy and Physiology*.

'The investigation of the reproduction of plants had never before made such rapid progress—and perhaps never since—as during the decade immediately preceding the publication of this article. Within this period fall the researches of Thuret, Cohn, A. Braun, and Naegeli, on the Algae: of Tulasne, de Bary, and Cohn, on the Fungi: of Hofmeister, on the Bryophyta: the discovery of the sexual organs of the Pseudophyta by Naegeli, Suminski, Hofmeister, and Milde: the investigation of the Gymnosperms by Gottsche and by Hofmeister, who proved that the organ called 'corpusculum' by Robert Brown is really an archegonium: the elucidation of the reproductive process of Angiosperms by Schleiden, von Mohl, Hofmeister, and Tulasne: and, finally, the bridging of the gulf between Cryptogams and Phanerogams, as also the recognition of

the alternation of generations in the life-history of the higher plants, the most memorable of all the brilliant achievements of Hofmeister. All this is faithfully recorded in Sir John Burdon Sanderson's article with an accuracy which shows with what interest he followed each step in advance. It may be doubted if, at that time, there was any one in this country, with the exception of Henfrey, who was so closely in touch with the marvellous progress of knowledge in this department of the science that we owe to the labours of the continental botanists before-mentioned; or who could have written such an account of it. However, the fact remains that this article is the best extant summary in the English language of the state of knowledge at that time.'

In 1854 Burdon Sanderson spent a short holiday in Scotland, in the course of which he visited Edinburgh and attended a meeting at his old resort, the Royal Medical Society. It is noted also in a letter that he ascended Ben Voirlich in search of a plant (probably interesting at the time in connexion with the article just referred to), which, however, he did not find. In the early part of the following year, in addition to his duties at St. Mary's, he was preparing for his examination for the Membership of the Royal College of Physicians, which he passed in March. His desire after this examination was to go to the Crimea along with other young members of his profession, in order to study medical and surgical practice under conditions very different from any which he had previously experienced. He relinquished this intention, however, in obedience to the wish of his parents, who were distressed at the thought of the dangers to which he might be exposed. Had his desire been fulfilled the course of his life might have been very different, for before the end of the same year he became a candidate for, and was soon afterwards elected to the post of Medical Officer of Health for Paddington.

Before entering upon this new phase of Burdon Sanderson's life it is desirable to call to mind the state of London at the time, particularly of those parts of it chiefly inhabited by the labouring classes, and of the conditions under

which up to that time they lived. In the interesting work on *English Sanitary Institutions*, by the late Sir John Simon, the chapter entitled 'The Reign of Queen Victoria' begins with these words:

'With regard to the subject-matter of these pages, the commencement of the reign of Queen Victoria was emphatically the beginning of a new era for England.... The old commonplace that without Health is no Wealth had been spreading in the national mind, as with the force of a new discovery; and the spreading of the old truth with so new a life had been in response to the progress which had been advancing from a century before in the resources of scientific medicine. Disease, always enough understood to be an evil, had gradually come to be seen as an evil which could often be prevented; and from the time when the community began to know that, with good government, it could, at its option, escape many terrible calamities of disease, Health necessarily began to take rank as an object of practical politics.'[1]

It was not until 1855 that the Act was passed 'for the better local management of the Metropolis', which enacted that 'every Vestry and District Board shall from time to time appoint one or more legally qualified practitioner, or practitioners, of skill and experience, to inspect and report periodically upon the sanitary condition of their parish or district, to ascertain the existence of diseases, more especially epidemics, increasing the rate of mortality, and to point out the existence of any nuisances or other local causes which are likely to originate and maintain such diseases and injuriously affect the health of the inhabitants; and to take cognizance of the fact of the existence of any contagious or epidemic diseases, and to point out the most efficacious mode of checking or preventing the spread of such diseases . . . and such persons shall be called 'Medical Officers of Health'.[2] London by this time was so awake

[1] Simon, *loc. cit.*, p. 178.

[2] It was also enacted that they should 'point out the most efficient modes for the ventilation of Churches, Chapels,' &c.; but this part of the Act seems to have become a dead letter.

to the necessity of coping with the serious evils existing within its borders, that the new law was complied with throughout the metropolis, and Medical Officers of Health were at once appointed. The Paddington Vestry was not behind the other districts of London, and Burdon Sanderson became a candidate for the post of Medical Officer. Although he had been scarcely more than two years settled in London he was encouraged to apply by influential people living in the parish, who assured him that if he chose to come forward he would certainly get the appointment. In his letter of application he referred to the opportunities he had had of becoming acquainted with the most important applications of medical science to the subject of public health ; and undoubtedly when completing his studies in Edinburgh his Dispensary practice led him to the poorest parts of the city, the conditions of which, to say the least, were not very different from those which then existed in London.[1] There were several other candidates, and one being a friend and colleague, he would not come forward until he had ascertained that this gentleman did not object to his doing so. Some of his supporters proposed to canvass the Radicals and others the Conservatives, so that his rivals should 'fall between two fires'. This, however, did not meet with Burdon Sanderson's approval, and he decided to withdraw from his candidature unless, as he expressed it, everything was 'straightforward and above board'. Happily the contest was conducted so as not in any way to offend his conscience or to interrupt friendly intercourse with his competitors.[2] When in February, 1856, the election was made, one of his rivals, Dr. Grailly

[1] Many years after he spoke of the condition of the poorer districts of Edinburgh about this time as being 'foul and filthy beyond anything that I have ever seen in London.'

[2] In his diary for January 21, 1856, it is noted that when he was in the City he suddenly remembered that it was the day for sending in applications for the office, so he 'hurried home to pack up his testimonials and take them to the Vestry Hall'. His thoughts had been more occupied with some scientific apparatus of which he was in search than with his immediate interests!

Hewitt, called to offer his congratulations, a circumstance which gave much pleasure to the successful candidate and was the beginning of an unbroken friendship.

The Medical Officer of Health for Paddington had a less hopeless task before him than was the case with some of his contemporaries in other districts of London. The Union contained a considerable proportion of good streets, the houses of which were occupied by single families. Even here, however, the drainage was very imperfect, and there were many small streets and courts where very insanitary conditions prevailed. It would be uninteresting to enter into the details of his life as Health Officer. His diaries are full of entries relating to inspections of dwellings where 'nuisances' of various kinds existed, and of visits to slaughter-houses, cow-houses, and bakehouses. His reports, which were every fortnight presented to the Sanitary Committee of the Vestry, as well as the quarterly summaries of his work, appear to have been destroyed, but the annual reports which were printed by the Vestry are sufficient to give an idea of the work he had undertaken.

One serious cause of unhealthiness in the district was the condition of the Regent's Canal Basin and that of the wharves upon its banks. These were the receptacle of the dust-heaps of Paddington, and the place where their contents were sorted and sifted. The disagreeable odours occasioned by this process led to constant complaints from the occupants of adjacent houses, and to the unwholesome emanations from the Canal Basin was attributed an undue proportion of infantile mortality in its neighbourhood. Year after year was this nuisance reported upon, and the repeated efforts to abolish it referred to. On two occasions when the Basin was emptied 'thousands of tons of black mud were removed'. But the foulness and the offensive odours occurred again and again. In 1865 some slight improvement was reported, and when in June of that year the water was once more let out the accumulation of mud was much smaller than on similar occasions previously. It was

also noted that there had been a marked improvement in the health of the inhabitants of the district in immediate proximity to the Canal, and particularly in that of the families of the bargemen. But this amelioration was temporary, and there seemed to be no means by which the Canal Company could be compelled to keep the Basin pure, if indeed such a thing were possible so long as refuse of every description was deposited upon the wharves, and much of it allowed to fall into the water.[1]

In a paper written in 1857 Burdon Sanderson gave further particulars as to the unwholesome nature of the Canal.

'For the purpose of measuring the influence which the Canal actually exercises upon the infantile mortality I some time ago distinguished, on a plan of the parish upon which every house is accurately marked, a district throughout which that influence might be supposed to operate with the greatest intensity, and which might be compared with the remainder of the parish. This district extends on either side of the Canal, reaching to a distance of about 200 yards. The whole of it is at an elevation differing little from that at the surface of the water, and about 100 feet above the mean sea-level. It comprised an area equal to about one-fifth of the whole parish, and contains a number of inhabited houses amounting to something less than two-sevenths. In the six summer months of the present year (1857), over which the observations in question extended, a total number of 252 children under five years of age died. Of these deaths 141 occurred on the banks of the Canal and only 111 in the rest of the parish. . . . In other words, there occurred from all causes under the age of five years a mortality of 8·3 to every 100 houses in the neighbourhood of the Canal, whereas in the rest of the parish the proportion was only 2·2.'

[1] Through the kindness of the present Officer of Health for Paddington, Dr. Dudfield, the writer has been permitted to see later Reports to the Vestry, from which it appears that ten years after Burdon Sanderson ceased to hold office, the Canal was still a source of trouble, 'the water turning putrid in the summer months' owing to defective cleansing, &c. (Report for 1874–5, by Dr. Hardwick, Medical Officer of Health for Paddington.)

He goes on to say that all collections of stagnant water appear to have a tendency to the production of a condition of the atmosphere similar to that of marshes, and that marshy districts have an extraordinarily injurious effect on the health of the neighbouring population, and, of these persons, infants and young children are the chief sufferers. Carefully chosen statistics had at the period under question been published which appeared to show the truth of these opinions, 'and made it easy to understand that any condition of the atmosphere approaching to that induced by marsh malaria is likely to be particularly fatal to children.' [1]

Another 'nuisance' occurred during his tenure of office, fortunately less serious and more easy of remedy than that of the Canal Basin. The Serpentine, which passes through Kensington Gardens and Hyde Park, became offensive, the odours which it emitted suggesting sewage contamination. Microscopic and chemical investigation proved this to be the case. The cause of the contamination of the Serpentine was found and removed, and during the years in which he was responsible for the health of the district the condition of the stream continued satisfactory.

There were several slaughter-houses in Paddington. The provisions of the Metropolitan Market Act, which came into force in 1856, gave the sanitary authorities the right of inspection, and enabled magistrates to refuse licences to those condemned as insanitary by the Medical Officer of Health. Many abuses were thus got rid of, but Burdon Sanderson was convinced that most of the evils could not be entirely removed until private slaughter-houses were made illegal and public ones were established at a distance from inhabited houses. This, however, was a counsel of perfection which has not yet been realized in this country.

[1] It is perhaps scarcely necessary to say that the above was written many years before the discovery of the relation of mosquitoes to marshes and marsh diseases, and even before it was known that water or moist surfaces, however evil-smelling, are not likely to disseminate disease germs in the atmosphere.

Bakehouses were also a frequent subject of report on account of their uncleanliness and insanitary condition. Sixty-five bakehouses were visited, 57 of them by the Medical Officer himself, and the horrible conditions under which the bakers lived and worked were exposed—conditions not only injurious to the health of the men, but disgusting in the preparation of the bread delivered to the public. Not much progress in remedying these evils could, however, be made until at the close of 1863 an Act was passed containing regulations as to cleansing and ventilation, and enacting that 'no rooms on the same level with the bakehouse shall be used as a sleeping-place unless separated therefrom by a partition extending from the floor to the ceiling and provided with an external glazed window'. No regulations were made, however, as to rooms on a different level, or in a separate building from the bakehouse, so that, however small, ill-ventilated, dilapidated, or otherwise unfit for its purpose such a sleeping-room might be, it was exempted from the operation of the statute.

In the laboratory of the late Dr. Bernays, at that time Lecturer on Chemistry at St. Mary's Hospital, many specimens of bread and of milk sold in the parish were analysed. As regards the former, the sole adulteration discovered was alum, and of this in half the specimens examined only a trace; and as a trace of the same substance was found on examination of the flour used in its manufacture, the whole blame could not be thrown upon the baker. Generally speaking, the amount of this adulteration was not sufficient to be detrimental to health. Where larger quantities of alum—from two to three grains to the ounce—were found, measures were taken under the adulteration Acts. It may be mentioned in passing that in 1857 Burdon Sanderson was taking part in a Commission on Adulteration which was then sitting at Whitehall.

As regards milk the results of analysis were more important. It was found that the amount of solid residue

was sometimes far below the normal. Pure milk must therefore have been correspondingly diluted by water or skim milk, or both together. As is shown in the annual Report for 1856:

'This adulteration is not the less important in its relations to public health because it does not consist in the addition of any deleterious substance foreign to the composition of pure milk. It cannot be too frequently repeated, that if we are to lengthen the lives of the people generally, it must be done for the most part by obviating those causes of disease which press so heavily upon infancy. The chief of these causes, as I have already endeavoured to point out, is deficient nourishment. If this be admitted, it must follow that the man who sells as pure milk a liquid which contains only a quarter of the proper proportion of solid nutriment is not only guilty of robbing the poor, but is engaged in a practice which is much more hurtful to the community than the addition even of poisonous ingredients to articles of food which are mere luxuries, e.g. pickles or confectionery.'[1]

The attempt to remedy the insanitary defects of houses, especially of those inhabited by the poor, is often, perhaps naturally, objected to by their owners. Such property is probably not very remunerative, and the proprietor hesitates to spend money in carrying out works which, though very important in the eyes of the Sanitary authority, do not seem urgent to him. The difficulty is increased when these proprietors are members of the Vestry. The Officer of Health for Paddington, in common with his colleagues in other parts of London, had his share of these annoyances. By tact and courtesy, however, they were frequently overcome without the necessity of spending tedious hours at the police courts, in enforcing the legal require-

[1] When in 1859 he was Secretary *pro tem.* of the Association of Medical Officers of Health he suggested an extension of the importation of milk from the country which, he said, 'the Association have reason to believe is by far more wholesome than milk derived from cows kept in London'. It was not till much later that the contamination of milk by disease germs was understood.

ments of the Vestry; occasions which were almost as much disliked by the magistrate as by the Medical Officer.

During the period in which Burdon Sanderson was Officer of Health London was twice visited by cholera. Two years previously there had also been an epidemic, which reached its climax in 1854. This began in the north of England, and was very deadly in Newcastle-on-Tyne, as to which his brother, Richard Burdon Sanderson, wrote to him: 'I do not wonder that you felt affected by the Newcastle accounts. It has been a fearful time, I assure you. We have lost above a thousand people in three weeks. Many have died of whom I knew something.' He goes on to relate how whole families were carried off, one after the other, and mentions that in the village of Bulman a colliery engineer, his wife, and one daughter with her husband and child, had all succumbed to the disease, and these were the only deaths in the village. 'My journeys into the town have been but melancholy,' he says. 'The continual sight of funerals must have been most depressing. In these, I understand, from the difficulty of getting people to assist, disgraceful scenes have often occurred. In Jesmond cemetery one hundred men could not dig graves fast enough.' As to the cause of the outbreak in that locality he can only suggest that it might be 'the general filth of the place, and I must say the scandalous condition of the water. I saw some laid on by the Company for all purposes, drinking included, into which you would have felt some hesitation about putting your hands.'[1]

With the Committee appointed by the Vestry of Paddington to superintend the Sanitary work of the district, Burdon Sanderson's relations were always pleasant. The members of it were invariably courteous, and desirous to carry out as far as possible any course of action which

[1] Many years later, when the writer of the above letter was taking an active part in public matters concerning the welfare of the town, he spent much time and labour in bringing about a complete reform in the water supply.

he recommended. The Vestry was a larger and more mixed body, but on the whole he got on well with them. Only in 1865, when he had been their officer nearly ten years, were they seriously in disagreement with him. In the spring of that year he was asked by the Government to proceed at once to the north of Germany to investigate the serious epidemic of cerebro-spinal meningitis, which then devastated the country around Dantzig. As his immediate departure was urgent he could not wait for the next meeting of the Vestry to ask for leave of absence, but requested the Chairman of the Sanitary Committee to explain the cause of his journey. The august body which presided over the welfare of the district were much affronted at his 'absence without leave', and at the next meeting they discussed at great length the conduct of their 'servant', as they considered their Medical Officer of Health to be, and while some members urged that the Vestry should feel itself honoured by their officer being chosen for such an important inquiry, others maintained that there were plenty of medical men with smaller salaries than £300 a year who were quite competent to have undertaken it. The discussion was reported at length in the *The Bayswater Chronicle*, which sided with those who defended him, and added that all who knew their officer would agree 'that he would fly to do good in any part of the parish, if he had wings to do so!'

At the end of 1866 he sent in his resignation to the Vestry, as he had by that time undertaken other duties which would require the whole of his time and attention; and his official connexion with Paddington ceased in the middle of the following year.

Concurrently with his strictly official duties Burdon Sanderson's thoughts were occupied by other subjects, some of which were suggested by his sanitary work, others more or less remote from it. Of the former were the investigations he made as to the ventilation of St. Mary's Hospital. Many evening hours were spent in the wards,

and sometimes the visits were continued far into the night, so that he might make himself thoroughly acquainted with all the processes by which the complete exchange of air was attempted; and their effects seemed to him to be quite insufficient. In 1862 he wrote a short paper for the Social Science Association, in which he pointed out that 'in the matter of ventilation guesswork ought to be superseded by measurement; that the success of every expedient tried should be judged of by exact instrumental observation of the result'; and that, as in the case of gas and water, the exact amount of supply in a given time ought to be regulated and recorded.

'It has been stated', he says, 'that as soon as a want is acknowledged, scientific people are ready to come forward with expedients. In the matter of ventilation, however, the want has not yet been felt, the necessity not yet admitted, and consequently people are still content with mere theories—such, for example, as that foul air has a particular tendency to ascend to the ceiling; or that, in general, air will pass more rapidly through wire gauze than through an opening of the same width. The very existence of these theories shows that the subject has not yet entered within the control of knowledge, for they are opposed to the most elementary principles of physics.'

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At the same time, and perhaps in connexion with these observations, he interested himself in the proportions of the constituents of atmospheric air under different conditions, and many specimens of air were examined. During a journey in Wales in 1855 specimens of air were collected on mountains and in valleys in order to compare them with those taken from towns and from various insalubrious localities.

A certain portion of his time was devoted to the practice of his profession. He was anxiously—for his peace of mind, too anxiously—concerned about his private patients; he loved his hospital work, and for many years this was continued after he had given up private practice. There he had more opportunity of studying diseases in their scientific

aspect, a subject which to the end of his life never ceased to interest him. It must not be supposed, as is sometimes alleged, that experiments in treatment to the detriment of the sufferers were made by him or other hospital physicians. But the more continuous study of morbid conditions which a hospital afforded revealed much that could scarcely have been learned by the occasional visits made to private patients.

He became a member of several medical societies. Two of these, the Chest Society and the Society of Observation, have become extinct; others, the Pathological, the Medical and Chirurgical, and the Clinical, still flourish as branches of the Royal Society of Medicine. All the spare time that he could legitimately use for the purpose was given to scientific work. Soon after he had settled in London he received a visit from his friend and teacher, Professor Hughes Bennett, who urged him not to be led away by the seductions of life around him to leave science for money-making practice. There was, however, no fear of this, as he loved science too well to forsake it. At this period of his life the subject of the mechanical and chemical processes of respiration was of special interest to him—a subject which occupied his attention for several years, the results being communicated to the Royal Society in 1866. This and other physiological investigations will be dealt with later.[1]

The duties of a Medical Officer of Health naturally led to the consideration of the prevention of disease and the investigation, so far as possible, of the sources from which it springs, and the conditions which favour its prevalence. Soon after Burdon Sanderson's appointment to this office his attention was directed to a disease well known in France,

[1] For the purposes of this investigation many pieces of apparatus were contrived, modified, or rejected for something more efficient. One of these he alluded to playfully in a letter to one of his sisters as his 'second wife. She is becoming more obedient and docile, and is losing her habit of always answering wrong and being a long time about it!'

but only beginning to prevail in certain parts of England. His commonplace-books in 1857 and 1858 are full of abstracts of memoirs, chiefly by French authors, relating to diphtheria. In the spring of 1859 he contemplated visiting a district in England in which it had become epidemic, and wrote to Mr. (afterwards Sir John) Simon, then Medical Officer to the Privy Council, asking his advice and help in this matter. This, although not his first introduction to the able and energetic officer who worked so strenuously for many years to advise the Government in sanitary matters, and to initiate and carry out scientific inquiries in the interest of public health, was the commencement of a friendship which was never broken so long as Sir John Simon lived. The beginning of their intercourse was in 1857, when Burdon Sanderson sent to the Medical Officer of the Privy Council a copy of his first Report to the Vestry of Paddington after his appointment as Officer of Health. Mr. Simon, in writing to thank the author, alluded to certain portions which had specially interested him, and asked for further information concerning them. The journey just referred to was apparently not carried out, but in June of the same year Burdon Sanderson was asked whether he would undertake a short inquiry for the Government as to an outbreak of diphtheria in Hertfordshire. In the winter of the same year and in 1860 he was directed to investigate various other counties in which the disease had been epidemic.

It may not be without interest to give a short account of the results of these investigations, as examples of the kind of effort which was made to trace the sources of epidemic diseases before the discovery of specific microbes. Diphtheria had only recently visited England, and it was looked upon in the various districts invaded as a new enemy, entirely distinct from any disease hitherto known. The ordinary weapons of medical treatment were found to be utterly inadequate—none of them seemed to have any effect.

The districts visited were as far apart from one another as Lincolnshire, Cornwall, Staffordshire, Northamptonshire, and Kent. The local features of the places varied considerably. In Lincolnshire special attention was directed to an alluvial tract of country on the west side of the Trent, which is bounded by that river and by the Idle and the Old Don. The last named was a navigable river before the seventeenth century, and the villages of Haxey, Epworth, Belton, and Crowle were at that time islands. The Old Don in 1859 was a stagnant ditch, but it still marked the boundary between the counties of Lincoln and York, and the villages mentioned were then surrounded by marsh land. They must formerly have been places of much greater importance than they were at the time of the diphtheria investigation. Haxey, one of the high villages, suffered severely from the epidemic, but less so than the hamlets on the low ground; and, strange to say, houses on the banks of the Trent and surrounded by marsh land escaped the disease altogether. In Cornwall, in the neighbourhood of Launceston almost all the villages attacked were in elevated situations. In the pottery district the town which suffered most severely was Hanley, built on a hill which overlooks the whole surrounding country, and said to be the highest town of its size in England. In Northamptonshire the village which suffered most was situated in a sheltered valley in an undulating country. In Kent diphtheria prevailed in thirteen out of twenty-four villages situated on the ridge of the chalk hills; but in the town of Sandwich, between the sea and the marsh land, only isolated cases were met with, and not a single case occurred in four villages built on the marsh.

It is evident that no definite conclusions could be drawn from these investigations, although carried out with the utmost care, seeing that the lie of the land, its geological conditions, the circumstances affecting the inhabitants of towns or villages as to drainage, overcrowding and dirt, were so diverse in localities where the disease prevailed.

Not until the discovery of the diphtheria bacillus twenty-four years later was light thrown on the causation of this, as of many other diseases.

Thirty years after these local investigations of diphtheria the subject of the causation of this disease was dealt with by Burdon Sanderson in the fourth of a series of lectures given by him before the Royal College of Physicians.[1] This discourse relates to the subject of specificity, and states that a specific disease, generally speaking, has more individuality, more unity than any other. From the moment, the lecturer says, that a disease can be regarded as not merely a complex of symptoms and lesions, but as the operation of a known cause which can be defined by the physicist, the chemist, or the biologist, its phenomena become part of a physiological process to be investigated, whether in the ward or in the laboratory, by physiological methods. He refers to a paper on the intimate pathology of contagion, written by him in 1869[2], at which time the ideas of pathologists as to the origin of infective diseases were vague. The laborious investigations of Chauveau had, however, already begun the new era, by proving as regards a series of communicable diseases in animals that the *materies morbi*—the substance which when introduced into the body of a susceptible animal gives rise to the process by which it was itself produced—is neither soluble nor diffusible, but consists of particles of indefinite minuteness, separable by subsidence or filtration from the liquid in which they are suspended. In 1869 Burdon Sanderson spent some weeks at Lyons, and had the opportunity of witnessing and taking part in Professor Chauveau's experiments. Subsequently he himself made many observations on vaccine, from which he drew the inference that these particles are probably organized beings and their powers

[1] The Croonian Lectures on the Progress of Discovery relating to the Origin and Nature of Infectious Diseases, 1891. Published in *The Lancet* in November of that year.

[2] Introductory Report: 'On the Intimate Pathology of Contagion.' *Reports of the Medical Officer of the Privy Council.* Twelfth Report, 1869.

of producing disease are due to their organic development. During the years which followed these researches very little progress was made in elucidating the relations between specific diseases and their causes, although this period was rendered for ever memorable in the history of the healing art by the triumph of Listerian surgery all over the world. No very precise knowledge of specific microphytes was acquired until Dr. Koch in 1876 made his first great discovery of the mode of growing the Bacillus anthracis outside of the living body, and of its remarkable life-history. A series of new facts were brought forward in subsequent years by him and by other scientific workers, but several years elapsed before light was thrown on the causes of diphtheria. The bacillus which is now known to be special to this disease was first recognized by Professor Klebs in 1883 as being constantly present in the superficial layers of the membranous exudation; and in the following year it was successfully cultivated by Dr. Loeffler in Berlin: it now goes by the name of the Klebs-Loeffler Bacillus. The poisonous substances or toxines were worked out by Roux and Yersin from 1888 to 1890, and in that way the proof of the causal relation of the bacillus to diphtheria was completely established.

Recent work has accentuated the view that the disease is spread mainly by personal infection, whether from recognized, unrecognized, or 'carrier' cases, and it is also believed that certain animals may act as transmitters of the disease to man. Milk has been shown to be a very important factor as a means of spreading the disease, whether from infected milkers or from the udders of infected cows.

The fact that the disease became more prevalent in England after sanitary conditions had considerably improved indicates that such conditions are not of the paramount importance which was formerly believed; while the association between school attendance and increased prevalence of the malady suggests that case-to-case infection is the

predominant factor. On the other hand, more or less periodic increases and decreases of the disease over wide areas suggest that other conditions exist which have so far not been thoroughly made out.[1]

In 1859 Burdon Sanderson applied for the post, then vacant, of Assistant Physician to the Brompton Hospital for Consumption. He was one of the two selected candidates, but some delay occurred in making the final decision, not apparently from doubts as to his professional claims, but because his rival was a stanch adherent of the Church of England. He was questioned on behalf of the electing body as to his religious principles, and was advised to get clergymen to testify to his orthodoxy! If any such testimony was sent it does not appear among his printed testimonials, but evidence was given by those with or under whom he had worked as to his conscientious discharge of and steady devotion to any duties he had undertaken. He was elected in December of that year.

Burdon Sanderson was no letter-writer. His correspondence was almost entirely connected with business matters or those relating to his work. Of this period we may quote one letter which is interesting as showing his attitude to religion at this time.

In 1853 he says:

'You have often, I am sure, heard it remarked by unthinking people that those that belong to, and especially those who at all devote themselves to, my profession very frequently become, sooner or later, materialists. Although this impression is an entirely false one, and is founded on an absurd misconception, there is no doubt that being constantly conversant with studies in which nothing is admitted otherwise than on the direct or indirect evidence of the senses, has a strong tendency to produce a sceptical habit of mind. The great mistake which we are apt to

[1] The writer is indebted to Sir Wm. Power, F.R.S., till recently Medical Officer to the Local Goverment Board, and to Dr. Ainley Walker, Lecturer in Pathology in the University of Oxford, for information as to the present opinions regarding the disease here dealt with.

commit is to apply the same standard to matters of faith as to matters of science. We cannot by searching find out God. I do not at all object to religion being called transcendental, nor does it at all diminish its reality to say that the region of faith lies beyond that of strict evidence.'

After referring to a book entitled *The Knowledge of Jesus the Best of the Sciences*, he goes on to say :

'That title has always appeared to me very objectionable, for it is the most distinctive characteristic of the knowledge of Jesus that it is not a 'science' at all. . . . I believe firmly that the only evidence of the truth of the Gospel which can stand against attacks, is that evidence which is derived from the inward feeling that there is something which draws us to God even against our will.'

CHAPTER IV

LATER PROFESSIONAL WORK

1860—1867.

The following five years were very busy ones for Burdon Sanderson. In addition to his usual professional and scientific work he was engaged by Mr. Simon to visit many counties in England for the purpose of ascertaining to what extent vaccination was efficiently carried out. As a result of his labours, and those of three other young doctors similarly employed, a minute survey of public vaccination was made in about 3,500 districts of England and Wales. By this means the Medical Department of the Privy Council amassed during the five years 1860–4 a complete system of information on which reforms could be based, the full particulars of which are set forth in the five annual reports (third to seventh inclusive) of its Medical Officer. Gratuitous vaccination for the poor was established by law in 1840, and vaccination was made compulsory in 1853, but this investigation showed that throughout the country there were flagrant evidences of carelessness and of unskilfulness in carrying out the duties of public vaccination—evidences, as Mr. Simon points out, especially discreditable in the country which had taught vaccination to the world. It was not until 1859 that the Privy Council required, 'strange as it may seem that no such requisition had yet been made, that persons to be in future appointed as public vaccinators should be persons who had learnt to vaccinate.'[1]

An outbreak of smallpox in the metropolis in 1857 naturally drew attention to the question of the neglect or otherwise of vaccination there, and even before the disease

[1] *English Sanitary Institutions*, pp. 382 et seq.

appeared in Paddington the Medical Officer of Health, who had then been little more than a year in office, made a careful investigation of the state of things in the parish under his control. He found that in two neighbouring districts, the population and other conditions being similar, there was a great contrast in regard to vaccination. In one of these the majority of the children had been vaccinated, in the other there was great neglect. When the disease invaded the parish there were few cases and no deaths in the former, whereas in the latter a considerable number of cases occurred and fifteen deaths. This contrast he attributed to the different action of the respective Registrars, one of whom co-operated actively with the Officer of Health in looking up cases of neglect, whereas the other neither did this himself nor aided the Medical Officer in doing it. In a paper communicated to the Society of Medical Officers of Health in November, 1859, some manuscript notes of which have been preserved[1], Burdon Sanderson drew attention to the circumstance that 'every one who has had the opportunity of observing the recent epidemic (of smallpox) must be convinced that even in London there exists a great deal of sham vaccination, so that in many instances the vaccination certified is really not vaccination at all, and not capable either of protecting the subject from the contagion of smallpox or of mitigating the violence of the disease'. He further alludes to the inefficiency of the public vaccination stations owing to the incompetence of so many of the vaccinators, and points out that any control except in the selection of the officers by persons scientifically competent to judge of their merits would be useless. 'Good vaccinators', he says, 'will

[1] Although these notes are not dated there is little doubt that they refer to this meeting of Medical Officers of Health. One of the resolutions passed by the Society, at a meeting when the paper was communicated by Burdon Sanderson, and recorded in their minutes, is, 'That every medical school should take means to instruct its students in vaccination'! See *Public Health, The Jubilee Number of the Society of Medical Officers of Health*, p. 30.

vaccinate well without either a central station or direct superintendence; bad vaccinators will do ill in spite of both.' He then suggests that the Medical Schools in London should either establish vaccinating stations in connexion with their respective hospitals, or a vaccinating room under the superintendence of the Hospital Staff, which would give the opportunity of instruction in the art of vaccination to the students.

No effort in this direction seems, however, to have been made, probably because in December, 1859, regulations were published by the Medical Department of the Government, authorizing certain public vaccinators to give certificates of competency to persons whom they had instructed, or of whose efficiency they had satisfied themselves by examination. The stations chosen were situated 'in places where there were recognized Medical Schools.' Only four such stations were assigned to London, a number which seemed inadequate for the purpose intended.[1]

In the course of the vaccination inquiry Burdon Sanderson visited 580 districts in 106 Unions in the counties of Essex, Suffolk, Oxford, Berks., Bucks., Gloucester, Wilts., Somerset, and Devon. The work was laborious and somewhat monotonous, involving a considerable amount of correspondence with the parochial Medical Officers, in holding interviews with them, in arranging visits to schools, where he had to examine the arms of the children to ascertain their condition with regard to vaccination, and in writing reports and other clerical work.

Almost invariably he met with the utmost courtesy from the doctors, even when, as he said, he had catechized one of them 'so pertinaciously that he, with the utmost good humour, was obliged to confess himself tired and to ask for an adjournment till to-morrow'. They generally responded readily to his request for an interview; only on one occasion was he disappointed, and he then attributed the absence of the doctors to the indifference of the clerk, who

[1] *Report of the Medical Officer to the Privy Council*, vol. ii, p. 14.

seemed little disposed to be obliging, and was very unresponsive to the request for information as to the condition of vaccination in his district. Although his vaccination journeys were not altogether devoid of attraction he was not sorry when, in 1864, they came to an end, and he was able, as he said, to exchange them for more congenial occupation.

Another subject which occupied Burdon Sanderson's attention about 1862 was the recovery of persons who had been asphyxiated by immersion in water or similar accidents. A committee to investigate this subject was appointed by the Royal Medical and Chirurgical Society, and reported in 1862, Burdon Sanderson taking a leading part in the experiments carried out under the auspices of the committee. Until these experiments were made very little was known as to the possibility of reviving asphyxiated persons by artificial respiration after the cessation of natural breathing, or as to the efficiency of various methods which had been proposed for carrying out artificial respiration.

One set of experiments was made on dogs or other animals, and showed that for some time after cessation of breathing an asphyxiated animal might be revived by artificial respiration. The heart continues to beat after all breathing movements have ceased, and during this period artificial respiration may be effective. In the case of drowning, however, the presence of water in the lungs is apt to make the artificial respiration ineffective, so that every effort should be made to remove this water.

In another set of experiments made on dead bodies the amount of air introduced by various methods proposed for artificial respiration was measured by an apparatus designed by Burdon Sanderson, with the result that the Silvester method was found to be most effective.

The methods adopted, and published broadcast, by the Royal Humane Society were based on this Report; and many lives have in consequence been saved. It is only

quite recently that an easier and more effective method of artificial respiration has been devised by Burdon Sanderson's distinguished pupil, Professor Schäfer, of Edinburgh.

'The year 1865'[1], says Sir John Simon, in a work already frequently referred to, 'was of extraordinary epidemiological interest. Early in that year rumours which were heard of strange diseases spreading epidemically in the north of Europe, made it necessary that the Privy Council, as quarantine authority for the United Kingdom, should be advised on the facts of the case, and, as a first step, that inspectors should be sent from the Medical Department to collect information in the accused Russian and German territories; whereupon, among other grounds for anxiety, the very disquieting fact became known that Cerebro-Spinal Meningitis—a febrile nervous affection of an extremely painful and dangerous kind, which we in England had hitherto hardly heard named, and which on the Continent had been but recently identified—was manifesting itself in epidemics in north-east Germany, chiefly about the lower Vistula, and that, only within the circle of Dantzig, at least a thousand persons had died of it in the last three or four months.'

Under these circumstances Burdon Sanderson was asked by Mr. Simon whether he could proceed without delay to Germany and report upon the epidemic. He lost no time, and before eight o'clock on the following morning was on his journey. He halted for a night at Cologne, where he at once began collecting information from medical men who had observed the disease. Next evening he arrived at Berlin, and, in accordance with his instructions, proceeded at once to the British Embassy. The Ambassador apparently was not at home, but Burdon Sanderson, somewhat to his dismay, was ushered, all dusty and travel-stained as he was, into the presence of Lady Napier. She, however, received him very kindly, and was much interested to hear what little he could yet tell her about the disease. On the following day he had two interviews with the Ambassador, and also visited the Minister of Public Instruction,

[1] *English Sanitary Institutions*, p. 289.

Professor Hirsch, and others, so that the day was fully occupied, and he felt that he had gained a great deal of information before night. In the evening he left by train for Dantzig, where he proceeded to call 'on the Commandant of the fortress, on the Land-Rath, the Medicinal-Rath, the Consul, and other dignitaries, and met with the most extraordinary kindness from everybody'. In the course of the day he saw a

'large number of cases of the disease, and had ascertained its leading characters . . . The weather', he said in a letter, 'is perfectly delightful, and the air so invigorating that although I travelled all last night, and it is now nearly twelve o'clock (midnight), I do not feel at all exhausted. The town is one of the most remarkable I have ever seen—quite mediaeval in its whole aspect, and very peculiar. There are enormous Gothic houses, like those in Ghent, many of them beautifully carved, and often ornamented with queer legends. The public buildings and churches are different in style from anything I have seen before.'

A fortnight was spent in Dantzig and the neighbourhood, visiting the hospitals of the town, and seeking out information as to the course, and, if possible, the cause, of the epidemic in the villages round about. These he inspected under the guidance of the Kreisphysikus, who was most kind and attentive to him, and who was well known and evidently popular in his district. At almost every halting-place refreshments were offered, of which 'Schnapps' formed an invariable item. It required all the Englishman's tact to refuse this, to him, unknown beverage, without hurting the feelings of his hospitable entertainers. In visiting the hospitals he was struck with their crowded and insanitary condition, and with the absence of efficient nursing.[1]

[1] Some years later when travelling in Germany, although not in the same district, he was astonished with the extraordinary contrast to his previous experience. The hospitals he then saw—one in particular at Halle—were extremely clean, airy, and apparently supplied with satisfactory nurses. The antiseptic treatment was scrupulously carried out, whereas in England at the same time there were still many hospital

Burdon Sanderson's evenings were spent in getting forward with his Report, in the vain hope that he might be able to complete it without much labour on his return to England. He left Dantzig on April 23rd, and, after spending a day in Berlin, travelled through to London, where he arrived in the morning of the 26th. For many days thereafter all the time that could be spared from his other duties was spent in preparing this Report, and it was not until the 17th of June that the last revise was dispatched to the Privy Council Office.

The summer of 1865 was notable in England in consequence of a severe outbreak of cattle plague, which spread with extraordinary rapidity to almost every part of the country. The results were so disastrous that on the 29th of September a Royal Commission was appointed to investigate the origin and nature of the disorder, to ascertain, so far as possible, the treatment best adapted for the cure of the infected animals, and to devise regulations to prevent the spread of the disease. Thus it came about that, shortly after his return from Dantzig, Burdon Sanderson was started on another important pathological investigation.

The Commissioners issued three Reports. The first, issued at the end of October, chiefly concerns the history of former outbreaks of cattle plague, and a statement of the first occurrence of the present epizootic, its rapid spread, and suggestions for checking its further extension.

The first cases in the outbreak in 1865 were observed in three English cows, purchased on June 19 at the Metropolitan Cattle Market, one being sent to Hackney and the other two to Islington. About a week later all were found to be diseased, and in a short time the cowkeeper at Islington lost her whole stock of 93 animals, as well as about a dozen which she had bought more recently. Early

physicians and surgeons who did not realize its importance, although this had been scientifically demonstrated by our own countryman, Lister.

in July a large part of the country was infected, and by October the epizootic had invaded twenty-nine counties in England, two in Wales, and sixteen in Scotland. At the middle of the month 17,673 animals were reported as having been infected, of which number 7,915 had died, and 6,866 had been killed. The source of the infection was somewhat obscure. The English cattle were supposed to have contracted the plague from some of a herd of 300 imported from Revel, but at the time these animals were shipped that place was free from the disease. It was conjectured that among these cattle there had been some which had come from infected districts, Steppe-murrain, as the disease was sometimes called, being always prevalent in some parts of Russia. Cattle murrain had visited this country on various previous occasions, notably in the fourteenth and fifteenth centuries, but the records of these invasions were inadequate to prove the identity or otherwise of the disease with the one now under consideration. The eighteenth century outbreaks were, however, better described, and there seemed no doubt that the epizootics of 1715, 1745, and 1865 were identical.

The proposals put forward by the Commissioners in their first Report consisted chiefly in recommending the prohibition for a limited period of the transport of cattle from place to place, and the slaughter at the port of entrance of all cattle brought from other countries. A minority of the Commission considered these proposals too stringent, and expressed the opinion that such an interference with trade was unjustifiable.

Regulations were, however, promulgated more or less in accordance with the Report of the majority of the Commissioners, but they seem to have been entrusted to local authorities, and to have been very imperfectly carried out. Perhaps as a consequence of this remissness, by the time that the second Report was issued at the beginning of February, 1866, the number of cases reported to the inspectors appointed for the purpose of watching the spread

of the plague had enormously increased, amounting now to 120,740; these reports were, however, supposed not to include the whole number of cases, as there was reason to believe that many were concealed from the inspectors. Meanwhile the Commissioners had initiated scientific investigations as to the nature of the disease, its pathology and morbid anatomy, the effects of different modes of treatment, of ventilation, disinfection, etc. These inquiries were entrusted to medical and veterinary experts, the results of whose work were given in the final Report dated May 1, 1866. The investigation allotted to Burdon Sanderson was the 'nature, propagation, progress, and symptoms of the disease'.[1]

The first intimation which he received of the desire that he should assist in these investigations was conveyed to him by Dr. Quain, one of the Commissioners, and he immediately set to work making arrangements for the purchase of cattle, and for a suitable locality for carrying on his experiments. He also made a bibliography of the subject, and his commonplace book of that date is almost entirely filled with abstracts of the writings of scientific observers in continental countries where epizootics of rinderpest had occurred. In a letter to his youngest sister on October 26 he says:

'I am just now so busy that I do not know which way to turn, and am likely to be so till the end of the year. I was very anxious to avoid this, but I have got an "inquiry" to do of a purely scientific character, and that I could not refuse. It will take about six weeks, and will not take me out of London.'

And on November 7 he writes: 'My cattle-plague business is now fairly begun. My difficulty was to get a place that would serve for the accommodation of my patients. I have now succeeded.' This was accomplished through the

[1] The other gentlemen concerned in the inquiry were: Dr. Murchison, Dr. Marcet, Dr. Bristowe, Dr. Lionel Beale, Mr. Varnell, Mr. Pritchard, Dr. Angus Smith, and Mr. (now Sir William) Crookes, F.R.S.

kindness of the late Professor John Gamgee, at the Albert Veterinary College in Bayswater.

'To-day the first patients are to come in, viz. four yearlings, four sheep, and two goats. These animals are to be observed during a period of several days in their normal condition, then inoculated with rinderpest, then again observed until the chapter of their sad existence closes. My investigation is called that of the 'natural history' of the disease, or in German 'das Wesen der Krankheit'—that is, the answer to the question what the disease is and does when left entirely to itself. All about the creature-patient is to be analyzed, weighed, measured, or minutely described as the case may be, so as to enable the human creatures surrounding to penetrate as deeply as possible into the causes and origin of its suffering. All this will be very laborious, but I need not tell J. that it is not the sort of work that is irksome to me, so that I do not grumble at it as I used to do at the vaccination work.'

During the progress of the inquiries the suggestion was made, and pressed upon the Commissioners in the public prints and elsewhere, that rinderpest and human smallpox were the same disease, and that therefore the former might be prevented or modified by vaccination. But experiment proved that the opinion was fallacious, for animals successfully vaccinated either by vaccine or by the virus of smallpox, when exposed to infection—whether naturally by contact with diseased animals, or by inoculation—contracted the disease just as readily as animals which had not been so treated; and the course of the disease and its fatal termination seemed equally unaffected.[1]

It is not necessary here to give an account of the symptoms of the disease, or of the observations made as to its course and the other questions referred to Burdon Sanderson for investigation. These are all recorded minutely, and may be read by those desirous of information on the subject, in the Appendix to the Third Report.

[1] As a result of Sanderson's frequent visits to the Smallpox Hospital at this time his wife, to his great distress, contracted the disease: it was fortunately not serious, being modified by original vaccination.

The explanation of the disease is thus stated in the Commissioners' Third Report:—

'A peculiar agent causes first of all a morbid state of the blood. Coincident with the first elevation of temperature, and of course long before there is the least outward appearance of ill-health, the blood of an animal which has taken the cattle plague contains an agent which can produce the plague in another animal. In other words, the earliest fact which can be made out after infection is that the blood contains the poison of the disease, so that serum obtained from it will give the disease by inoculation. This fact, ascertained by Dr. Sanderson, is the most important pathological discovery yet made in cattle plague. It is pregnant with consequences in medical doctrine, for though the existence of a similar fact has long been suspected in several human diseases, it has never been proved in any. So material, indeed, is it that we must dwell on it for a moment. The poison contained in a minute portion of the mucous discharge from the eyes and mouth of an animal ill with cattle plague, if placed in the blood of a healthy animal, increases so fast that in less than forty-eight hours, perhaps in a far shorter time, the whole mass of blood, weighing many pounds, is infected, and every small particle of that blood contains enough poison to give the disease to another animal. This at once accounts for the rapid spread of the cattle plague.'[1]

For five months this piece of work had occupied all Burdon Sanderson's available leisure. Twice a day visits of inspection were made to the patients in the Veterinary College, and not infrequently he passed five hours in the day with the animals.

Thirty years later than the period now under consideration Burdon Sanderson offered a copy of the Commissioners' Report to Professor Koch, who was about to proceed to

[1] In some short biographical notes, relating to his scientific work, Burdon Sanderson says: 'My first investigation in experimental pathology related to the nature, progress, and mode of propagation of cattle plague. The most important new fact discovered was that, in that disease the serum of the blood is capable of communicating the disease by inoculation, but contains the virus in such form that it is incapable of passing through an animal membrane.'

South Africa to report upon rinderpest, which was then prevalent in that country, and whose acquaintance he had made in Breslau in 1877. Dr. Koch's reply, a translation of which is given below, is interesting as showing the value he placed on the work done by Burdon Sanderson, and the friendliness which animated them both, although of late years their opportunities of meeting had been rare.

'Grosvenor Hotel, London, Nov. 14, 1896.

'Dear Sir,

'Your work on cattle plague was long ago known to me as the best aetiological and pathological investigation of this disease that has been made; and I at once provided myself with a copy from the library of the Royal Veterinary School in Berlin, when I undertook the task entrusted to me by the Government of Cape Colony. I had previously studied it very thoroughly, and I am taking it with me in order to be able to refer to it. It will accompany me on my journey and will always be a true and reliable counsellor. As I am already provided with the Report, I return the copy that has been kindly placed at my disposal, as it is probably the only one in your possession, and of much value to you. But I must tell you that your offer has deeply touched me. I see that you still preserve towards me that friendly feeling which you have in former times so often shown to me.

'Accept my cordial thanks, and do not forget

'Yours most faithfully,

'R. Koch.'

As soon as the cattle-plague investigation was concluded and reported on, an outbreak of cholera occurred in England, and although Paddington was less severely affected than other parts of London, Burdon Sanderson's duties as Officer of Health were considerably increased by it. Moreover, he was engaged by the Privy Council, through Mr. Simon, to make a series of experiments as to the communicability of cholera to animals. His note-books show that, as usual, he made himself acquainted with the work that had been already done by others, and he found that in previous epidemics on the continent of Europe, German and Italian investigators had attacked this subject, but for the most

part with negative or inconclusive results. Similar efforts were made in this country in 1849, but even on that occasion the results were doubtful, although cases had been reported of dogs which had voluntarily swallowed infective material from cholera patients, and had died rapidly of a disease, the symptoms during life and the appearances after death being similar to those observed in human sufferers. He also studied the history of the origin and spread of the disease, which many years later he communicated to the public in a lecture given at the Royal Institution on 'Cholera : its Cause, Nature, and Prevention'.[1]

He there states that the first serious epidemic of which records are known occurred in India at Jessore near Calcutta in 1817, at which time the disease was looked upon as an entirely new malady, although it was already known as an annual visitant in Bengal. It spread with great rapidity to places as far distant from one another as Patna and Dacca, and in two months from the first outbreak had reached Benares.

'In October [of the same year] an event occurred which at once gave the disease a significance it had not before possessed. The Marquis of Hastings, with an army of over ten thousand Europeans and a much larger native force, was in the Bundelkund, not far from Allahabad, where cholera was then raging. Cholera had on several previous occasions interfered with military operations, but this time it attacked Hastings's European troops with a violence of which there had before been no example. The pestilence continued for several weeks with unabated destructiveness, until early in November the army was withdrawn from the Bundelkund and moved westwards in its march towards Gwalior, on which the mortality at once subsided. Thousands of dead and dying were left behind, but cholera was left behind with them, and a lesson was learned which has since been often repeated in Indian experience—that when a military force is encountered by cholera, removal from the infected locality is the only effectual way of checking it.'

[1] This lecture was afterwards published in the *Contemporary Review* for August, 1885. The passage quoted has been taken from the article.

In the next two years the disease had spread over nearly the whole of India, and a little later it came to Ceylon, and afterwards to China, then to Mauritius and the east coast of Africa: Java, the Celebes, and the Philippines were invaded at about the same time. In 1822 or 1823 it came into Europe, the first town attacked being Orenburg in Russia. After that date it appears to have been absent from Europe until 1829, when it again broke out in Orenburg, and by the following year it had advanced further westward, and towards the end of 1831 it made its first appearance in England at Sunderland, Newcastle, and Shields. In 1832 it had come to London; and in June of the same year it made its first appearance at Quebec in a lodging-house occupied by Irish immigrants, who it was believed had conveyed the disease across the Atlantic. From there it spread to other parts of Canada, to the United States, to Cuba, and to other Spanish-speaking countries of the New World. In 1847 it again invaded Russia, spreading through North Germany to Hamburg, and to England. For about six years the disease was again absent from Europe, but in 1853 there was a fearful epidemic in St. Petersburg, which travelled to this country. After this there was a truce of a dozen years in Europe, but in 1865 the disease once more appeared—this time beginning its progress from the shores of the Mediterranean. Thence it travelled into Germany in 1866, and the Prussian army after its short campaign in that year encountered it in Halle and Leipsic, and is said to have suffered so severely that more soldiers' lives were lost by cholera than by the weapons of the Austrians.

It was quite natural that scientific men in this country should be desirous to investigate the disease and attempt to decide the question of communicability which had been left unsolved by previous researches. Burdon Sanderson's experiments were begun on August 11 and continued till the end of September. In the Report to the Privy Council they are minutely described and tabulated, and they were

evidently made with extreme care. He says of those experiments, in the manuscript notes already referred to, that after cattle plague his next

'serious pathological work was on the communication of cholera to animals. . . . The purpose of the inquiry was to determine the conditions under which the alvine discharges of cholera patients attain the greatest virulence. The method consisted in feeding animals with extremely minute quantities of the morbific material. A large number of experiments were made, but the further progress of the investigations was prevented by the decline of the epidemic.'

A few words may fitly be said here regarding the Clinical Society, the inauguration of which took place at a meeting which had been convened by Burdon Sanderson at his house in Queen Anne Street on October 29, 1867. A considerable number of members, particularly of the younger members of the medical profession, assembled to consider the desirability of starting a new medical society to be called the Clinical Society, which was to be not a rival but a sister society to the Pathological. The latter he considered had been too exclusively occupied with the examination of post-mortem results, especially of such as were unusual, and he believed that a new organization having for its object the cultivation of that part of pathology which relates to disease during life would be of great benefit to the profession. As he expressed it in a letter to Sir Thomas Watson, then the *doyen* of the medical profession:

'The practical ends which the promoters have in view are to facilitate and stimulate the investigations of the many undetermined questions in clinical medicine and surgery which require the combination of many observers for their solution—to render available the immense field of observation offered by the London hospitals for this purpose, and lastly to induce young physicians and surgeons, during the first ten years of their professional life, to devote themselves to the systematic working out of clinical or therapeutical inquiries, rather than to the collection of rare pathological curiosities.'

Sir Thomas Watson agreed to give his countenance to the new Society and to be its President. The first meeting took place on the 10th of January, 1868, when he delivered the inaugural address, in which he expressed himself as follows:

'The Society we are founding to-night seems to me well calculated to bring about that which, in my judgement, is the thing most needful at present among us—I mean, more exact knowledge, and therefore more direct and intelligent purpose, and more successful aim in what is really the end and object of all our labours—the application of remedies for the cure or relief of disease.'

After pointing out several lines of inquiry which he considered specially desirable, he ended by expressing his belief that the Society, 'if wisely and strictly managed, will hereafter be spoken of as the starting-point of a vast and solid improvement in that which is our special office in the world, the scientific and intelligent exercise of the divine art of healing.'

The President, the twelve Vice-Presidents, the Treasurer, and the two Honorary Secretaries, all men of mark in their profession, have ceased from their labours. Of the 127 original members about twenty still live, and a large proportion of them bear names well known and honoured as physicians and surgeons.[1]

[1] In answer to some inquiries made by Lady Burdon Sanderson Dr. Hale White kindly informed her that the Clinical Society undoubtedly fulfilled the objects for which it was founded. The large attendances at the meetings, and the number and variety of subjects proposed for discussion, testified to the keen interest taken in it by the members to the end of its separate existence. The Society was well to the front with every advance in science bearing upon medicine, such, for example, as the X-rays, the first medical demonstration of which was given to the Clinical Society. It died, not from inanition, but because almost all the Medical Societies of London were recently amalgamated into the 'Royal Society of Medicine'.

CHAPTER V

EARLY SCIENTIFIC WORK IN LONDON

In 1867 Burdon Sanderson resigned his appointment as Medical Officer of Health. His father had died two years before, and his circumstances, so far as money was concerned, were improved. But in addition to this it was clear that his inclinations were towards scientific work, pathological and physiological, and that he desired to have more time to devote himself to this. He had almost arrived at the age of forty, and in a day when appointments of the kind he was capable of filling were far from numerous, it was perhaps difficult for him to judge what his immediate future would be. He was of course more than fully occupied. He was lecturing on Pathology at the Middlesex Hospital, and he held posts as Physician both there and at the Brompton Hospital. He was likewise occupied at the Albert Veterinary College with his work on cattle plague, and was writing various articles for medical journals. He was also working at the sphygmograph and its applications in the study of disease. On this subject he published a short but valuable book in the year 1867. In reality, however, his mind was tending in the direction of pure Physiology. About 1866 he began an investigation on the effects of respiration on the circulation; and in 1867 he was not only elected a Fellow of the Royal Society, but asked by the Society to deliver the Croonian lecture for the year on the results of his experiments. This signal recognition probably influenced him greatly. But it was necessary that he should work experimentally in a laboratory, and this opportunity he gained through the friendship he had formed with Dr. Sharpey, who made arrangements for him to work

in the limited space then available in the laboratory at University College.

The year 1870 was in a certain sense the beginning of a new departure in Burdon Sanderson's life. He then severed his connexion with Middlesex Hospital, where he had been Assistant Physician for some years, and a few months later he resigned the post at the Brompton Hospital for Consumption which he had held for more than ten years, first in charge of the out-patients, and later on as full Physician. He also abandoned his private practice, which had never been large.[1] It was not any decline in his interest in the science of Medicine which led to this step. The study of disease attracted him to the end of his life, although latterly he viewed it through the work of other observers. His mind, however, was teeming with subjects which seemed waiting for investigation, and which he could not hope to deal with unless he were relieved of his medical work. As in all else undertaken by him, his whole heart was given to the task in hand, and nothing was done in a perfunctory manner. He had done good work in the Middlesex Hospital Medical School and at University College. But he felt that in a laboratory under his own control he would be more at liberty to carry out the series of investigations which were now occupying his attention. He therefore hired a room for the purpose, which is said by Dr. Ferrier, who worked with him there, to have been 'a poor affair—nothing more than a room over a stable in Howland Street.' Much valuable work, in which Lauder Brunton, Ferrier, and Klein participated, was, however, there accomplished.

Sir Lauder Brunton, Burdon Sanderson's almost life-long friend, writes of these early days :

'I have been asked to furnish some information regarding

[1] It was in January, 1873, that he was requested to assist at the autopsy of the late Emperor of the French. This was perhaps the last work of the kind that he conducted.

Burdon Sanderson's work in Howland Street, and it is to me both an honour and a pleasure to do so, but in order to explain why he set up a laboratory in Howland Street, I must say a few words about the nature of his work. It is impossible for any one to understand the enormous influence he exercised upon Physiology and Pathology if one looks only at his original discoveries. Great and important as those were, the main work of his life may be said to have been that he entirely revolutionized both teaching and research in Pathology and Physiology in this country. He changed the teaching from didactic into experimental, and while before his time research was almost entirely confined to microscopical work, he introduced experiments of all kinds. When, with the aid of a tin-plate worker, he first made, in 1865, the kymograph, with which he performed his experiments on the relation between the respiration and the pulse, there was probably, with the exception perhaps of a half-dozen sphygmographs, no physiological recording instrument in use in this country. At his death, every University and Teaching School of importance was provided with a fully equipped laboratory.

'This alteration he effected partly by his own example, partly by the publishing of his *Handbook for the Physiological Laboratory*, and partly by the kind help, teaching, and direction which he gave to others, especially younger men.

'It is difficult for me to express how much I owe to Sir John Burdon Sanderson. I came up as a stranger to London, only bearing a letter of introduction to him, but he took me into his house, advised me what to do, and after deciding that I should study abroad, he obtained for me while I was away a Lectureship at the Middlesex Hospital, and after my return gave me every assistance to carry on laboratory investigation, and finally asked me to collaborate with him in the preparation of his *Handbook for the Physiological Laboratory*. Nor was I the only one whom he treated in this way. He invited Dr. Ferrier, who was then engaged in general practice, to come to London as his assistant, and obtained for him the Lectureship on Physiology at the Middlesex Hospital, which he himself vacated. Recognizing exceptional ability in Professor Schäfer, who was then a student, he led him to give up all idea of practice and devote himself to Physiology.

Besides Sanderson himself, Ferrier, Schäfer and I were all working in Howland Street Laboratory in the summer of 1870. This laboratory was started because Sanderson had begun to devote himself very much to experimental pathology, and the work could not be done either in the Middlesex Hospital or University College. During the summer Professor Stricker came over from Vienna and worked also in this laboratory, and at his recommendation Sanderson afterwards invited Dr. Klein to come over and settle in this country.

'The great advance which pathological research has made in the last twenty years in this country is greatly due, I may say mainly due, to the impulse which Sir John Burdon Sanderson gave to it. The increase in our knowledge of Pathology has been so great that it is very difficult for the younger pathologists at the present day to realize the condition of the science when Burdon Sanderson began to work at it. We are so familiar now with various kinds of disease germs, microbes of all kinds, bacilli, bacteria, and cocci, that we hardly remember that when Burdon Sanderson sent in his Report in 1869 "On the Intimate Pathology of Contagium" to the Medical Officer of the Privy Council the question whether contagia consisted of definite particles or not had not been settled. In that report he arrived at the conclusions: (1) that every kind of contagium consisted of particles, and (2) that these particles are organized beings, and their powers of producing diseases are due to their organic development; (3) that within the body of the infected individual the particles of contagium rapidly reproduce themselves, while out of the body they are capable of resisting for very long periods the causes or conditions which, if not restrained by organic action, would produce chemical decomposition.

'In 1870, with the assistance of Dr. Ferrier, he continued his research into the nature of contagion, and amongst other work done at Howland Street he had to consider the question of spontaneous generation. Without entering upon this subject in general he proved that, as regards animal liquids and tissues and the liquids used as tests for the presence of microzyme germs, no spontaneous evolution ever takes place.

'The results obtained by Sanderson's work at Howland Street may not seem to be imposing, but they were some of the foundation stones upon which the present structure

of Pathology is raised, and so thoroughly and accurately did Sanderson lay them that they remain as firm now as they did forty years ago.'

While Burdon Sanderson was in the midst of other investigations, in the summer of 1869 he visited the Amalgamated Clifford Mines in Cornwall in order to investigate the effects of great heat on miners. This visit was made at the request of Sir W. (afterwards Lord) Armstrong, who was concerned as to the effects of the heat on the health of the men, and also considerably puzzled as to how they withstood the excessive moist heat reported to exist at certain points in these mines.

The hottest places were, according to Burdon Sanderson's notes (which were never published), about the 235 fathoms level, where water at a temperature of 120° F. was flowing from the rock. Armed with various thermometers, a Regnault hygrometer, and a sphygmograph, he descended the mine and examined the working places and some of the men at the end of their shift. He found that the air itself had a temperature of 103°, and that it was completely saturated with moisture. Under such conditions it was absolutely impossible for a man to exist for any considerable time without a serious, and probably fatal, rise of body temperature. It appeared, however, that the men worked for only fifteen minutes at a time in the hot air, and then came out into the air-current at a temperature of about 80° to cool. Some of them had been working for many years at this part of the mine, and had remained perfectly healthy. Burdon Sanderson found that when they had completed each short spell of work their temperatures had risen only a degree or two at most, the pulse also being somewhat increased in frequency. After half an hour's rest in the cooler air both temperature and pulse were perfectly normal. Tracings obtained with the sphygmograph after rest showed a perfectly normal condition. He came to the conclusion that under the existing conditions no injury to health was produced, as the men came out before the rise

in their body temperatures was sufficient to cause harm, and the great discomfort produced by the heat was quite sufficient to warn them. It was clear, however, that their working efficiency was greatly reduced, as they could only work for short spells. In a letter to Sir W. Armstrong, supplementing the report which he sent, he expresses the opinion that in all probability men cannot work continuously at a temperature of over 90° in saturated air. In this opinion he was certainly well within the mark, as was definitely shown nearly forty years later by his nephew in the course of a further investigation in connexion with Cornish mines.

The means of descent and ascent at the Clifford mine was the now old-fashioned 'man-engine', of which one still exists in England, at the famous Levant Mine not far from Land's End. A man-engine consists of a series of wooden beams, connected together by iron plates, and extending from the top to the bottom of the shaft. This structure is balanced at intervals by huge balancing-bobs, so that it is suspended in the shaft and free to move up or down about ten or twelve feet; and is kept in constant slow movement up and down by a steam engine on the surface. To the beams of the man-engine small platforms, large enough for one man to stand on, are fixed at regular intervals, and platforms at corresponding intervals are arranged at the side of the shaft, or on another man-engine alongside the first. A man descends by stepping on to a platform as it comes up to his level, and when this platform has descended with the next stroke of the engine he steps off on to the platform on the side of the shaft, or on the other man-engine. By repeating this process again and again he easily descends to the bottom of the shaft; and of course he can ascend with equal ease. Each man takes a considerable time to ascend or descend, but there is no waiting at the top or bottom, so that in reality the men can get up or down without much delay.

After completing his investigations at the 235 fathom

level, Burdon Sanderson stepped on to the man-engine in order to ascend the shaft. It appears that he immediately became faint and dizzy, and could only steady himself by clinging on to the hand-rail. It was a dangerous position. The huge unwieldy machine went up and down with relentless regularity, and in those days there was no telephone or other rapid means of communicating with the engine-man. As the sides of the narrow platforms of the man-engine come within about an inch of the platforms on the side of the shaft, fitting almost like trap-doors, failure to stand erect might easily result in a limb being crushed, or some worse injury. After some time Burdon Sanderson got off safely, but the experience was a very unpleasant one. In all probability his own temperature had, without his knowledge, risen considerably while he was investigating the hot workings. Visitors to the hotter parts of Cornish mines are very apt to be affected in this way, and to find themselves in such a condition that any exertion quickly produces a state of collapse. Some well-meaning mine official has perhaps cautioned them against the effects of 'chill' on coming out, and recommended them to wear a thick flannel coat, with the inevitable result that the body temperature rapidly rises.

The miners on the spot appear to have told Burdon Sanderson very candidly that his mishap was due to his new-fangled instruments, which were 'quite unsuited' to a Cornish mine! At that time even a clinical thermometer was altogether a novelty.

In 1871 there was founded in London the first Pathological Laboratory that ever existed in this country, and there Burdon Sanderson made many of his experiments on infective processes. It was named the Brown Institution after Mr. Thomas Brown, who in 1852 bequeathed the value of certain stocks and the residue of his personal property to the Chancellor, Vice-Chancellor, and Fellows of the University of London, 'for the founding, establishing, and upholding an Institution for investigating, studying,

and . . . endeavouring to cure maladies, distempers, and injuries any Quadrupeds or Birds useful to man may be found subject to.' The University was to appoint a Professor or Superintendent of the Institution, and a Committee of their own body, or of medical men, 'to control the number and cases of diseased or injured animals or their carcases, for the promotion of science'. To the Professor or Superintendent was assigned the duty of giving annually 'on the business of the said Institution at least five lectures in English and free to the public'. The money so bequeathed was to accumulate for several years, but if the University of London should 'omit to have such Animal Sanatory Institution founded and established within the space of nineteen years from the time of [his] death', the whole of the properties were to go to the University of Dublin for the founding and maintaining Professorships in three or more of the following languages, namely Welsh, Sclavonic, Russian, Persian, Chinese, Coptic, and Sanscrit.

The University of London formed a Committee, and a Trust for the purpose of considering the best means of carrying out the conditions of the will, in the summer of 1869, and many meetings were held during that and the following year. In the autumn of 1870 the site for the Institution was considered, and in the course of the following year it was decided upon, plans for the building being agreed to. Burdon Sanderson was appointed the first Professor Superintendent, and towards the end of 1871 the Howland Street Laboratory was abandoned, its contents being removed to the Brown Institution in the Wandsworth Road.

In an article in *Nature*, December 21, 1871, credit is given to the University of London for the scheme devised by the Senate for carrying out the conditions of the will:

'Happily alike for humanity and for science the late Mr. Brown showed by his selection of persons to be entrusted with the carrying out of his intentions, by the

instructions contained in his will for their guidance, and by the terms in which he defined the purposes of the proposed Institution—placing study and investigation first, cure afterwards—that he was not actuated by a mere sentimental sympathy for the lower animals as such, but that he desired, by promoting the scientific study of their diseases, to benefit mankind.'

Among the Committee of Directors were names well known and esteemed in science and medicine, such as Sir James Paget, Sir John Simon, Dr. (afterwards Sir George) Buchanan, Sir William Gull, Dr. Richard Quain, and Dr. Sharpey; 'of each of whom it may be said that he has contributed a large proportion to the total amount of work done in his own branch of science in England during the past thirty years.' The writer of the article adds:

'Believing that the study of Pathology, like that of Physiology, of which it forms part, can only be successfully prosecuted by observing the operation of chemical and physical laws in the living diseased body, and applying the same methods as are used by the chemist and physicist to their investigation, and that the more this principle is acted on, the more rapid and solid will be the progress made, we regard the establishment of the Brown Institution as an important step in the right direction.'

Burdon Sanderson began his work in the new Institution in January, 1872, and resigned his appointment at the end of 1878. Much, if not all, of the work embodied in his Reports for the Medical Department of the Local Government Board was carried out in the laboratory.

As regards the condition in Mr. Brown's will, that the Institution should investigate and study the diseases of animals useful to man, the resources of the Committee were insufficient for the purchase and housing of large and valuable animals. This difficulty was overcome in 1875 by an agreement between the Committee of the Brown Institution and the Veterinary Committee of the Royal Agricultural Society, the latter body having

voted £500 for the expenses of such experiments. Researches on foot-and-mouth disease were at once begun, and, shortly after, investigations as to infection and other circumstances in connexion with an outbreak of anthrax (or splenic fever), which occurred in Lincolnshire in February, 1878. The Reports of these series of observations were communicated to the Royal Agricultural Society.[1]

In an address given many years later to the Royal Medical Society of Edinburgh Burdon Sanderson alluded to his connexion with the Brown Institution, the time during which he held the appointment of Professor Superintendent being, as he remarked, eventful in the history of Pathology. 'The Franco-German war had taught a great lesson in antiseptic surgery, the result of which was the triumph of Listerism all over the world, later in London than anywhere else. We had learnt that tubercle and inflammation were infective processes, and that in the latter the infection was due to micro-organisms, and soon after the Streptococcus, with which you are now so familiar, was discovered in the spreading border of erysipelas.' After referring to Koch's discoveries, he added the names of some of his successors at the Brown Institution, the first in the list being Dr. Greenfield, who now holds the Chair of Pathology in the University of Edinburgh. Then followed the late Dr. Roy, afterwards Professor of Pathology at Cambridge, Sir Victor Horsley, and Dr. Sherrington, now Professor in the University of Liverpool. 'As myself the founder of the Brown Laboratory,' he continued, 'I think you will admit that I have some ground for gratification in mentioning the names of men who have, without exception, done signal service to the science of Medicine.'

In consideration of the fact that subsequently Burdon Sanderson devoted his attention mainly to Physiology, Dr. James Ritchie has kindly written a short review of his

[1] *Journal of the Royal Agricultural Society of England*, vols. xii, xiii, xv, xvi.

contributions to Pathology, more especially on its experimental side.

'During the performance of the administrative duties with which in the early sixties Burdon Sanderson as Inspector to the Medical Committee of the Privy Council was charged, his mind, with its constant bent towards scientific inquiry, must often have been at work on the problem of the etiology of infectious disease. This is manifest from his reports. Thus, in that on the prevalence of epidemic cerebro-spinal meningitis in the valley of the Vistula, he recognizes the difficulties in the epidemiology of this disease which even at the present day are unsolved; as, for example, its appearance in different parts of a community without contact or a common source of infection being traceable between the sufferers. The early interest which he exhibited in that branch of Pathology, to which later he was to devote so much attention, no doubt led to his being asked in 1865 to take part in the work of the Experimental Committee of the Royal Commission on the Cattle Plague, and his inquiries here constitute his first personal contribution to the elucidation of the infective processes. Certain noteworthy results were obtained. It was shown that in artificially inoculated animals, whenever the earliest manifestations of disease were present, the blood was already infective for other animals; and commenting on this discovery, the Commissioners say: "It is pregnant with consequences in medical doctrine, for though the existence of a similar fact has long been suspected in several human diseases it has never been proved in any." Very important from the point of view of the development of Burdon Sanderson's ideas on infection were his own observations that the infective agent of the disease did not diffuse through parchment paper; and those of his colleague, Dr. Lionel Beale, who, working with a microscopic magnification of 2,600 diameters, failed to find any definite formed objects in the blood. The former fact led Sanderson to the conception of infective agents as particular bodies and not substances in solution, and the latter, there is reason for thinking, made him for long entertain the possibility of some infective agents being ultra-microscopic—a position which is now generally accepted.

'About this time a new development took place in the

work of the Medical Committee of the Privy Council, which was then being organized into a separate department under its present name of the Local Government Board. Its chief medical officer was Dr. John Simon, one of the ablest and most far-seeing of modern Public Health Administrators, who in his Report for 1865 announced that his official superiors had determined "that in future there should to a limited extent be conducted under their auspices such investigations in abstract pathology as might seem likely to be of ulterior effect on the more immediately practical and popular objects of the department." There was thus inaugurated the long series of purely scientific researches which ever since have been carried out for the administrative Public Health Department of this country, and in which from time to time Burdon Sanderson during the following twenty years took part.

'The first subject to which he directed his attention was that of the infectivity of tuberculosis. A common tendency of the period was to regard this process as one of constitutional origin, whose morbid effects in the body were of a specific nature not to be classified with such processes as were grouped under the term inflammation. In 1865 Villemin had undermined this position by showing that tuberculosis could be communicated to the lower animals by inoculation with tubercular material derived from man, and Burdon Sanderson in 1867 confirmed these results—at the same time showing that from the bodies of guinea-pigs, thus made tubercular, other guinea-pigs could be likewise infected; it is to be noted, however, that he puts forward no views as to the nature of the infective agent. A further feature of his work was a careful microscopic investigation of the anatomical lesions originated, in which it was shown that these possessed the same characters in different parts of the body, and further were indistinguishable from those occurring naturally in man. In addition to proving the identity of tubercular lesions, whether arising naturally or from artificial inoculation, there emerged from this research an important series of facts whose significance the author, as time went on, insisted on more and more, namely, that the tubercular process was essentially one affecting the connective, and especially the lymphoid tissues of the body; that in such tissues in tuberculosis a proliferation of the cells normally present occurred, and that the gross naked

eye changes were explained by this proliferation and by a subsequent degeneration of the newly formed cells. These observations militated against two views which had been held, namely, that which looked on the presence of a "tubercle cell" as characteristic of the tubercular process, and that which made this process analogous to what occurs in tumour formation. The way was thus opened up for the modern view that in tuberculosis we have to deal merely with an irritant of a special kind, giving rise to changes analogous to those caused by many other like agents. In this research Burdon Sanderson also made important observations on the mode of dissemination of the tubercular infection once this had been established. He pointed out that there was an absorption of the poison by the lymphatics, and a consequent infection of the nearest lymphatic glands; he held that an infection of the systemic circulation through the veins might secondarily be brought about, and that when any bodily organ became infected by this channel its lymphatic glands might in turn show tubercular changes; he further showed that, when systemic infection occurred, the serous membranes were specially apt to show tubercular lesions. Modern investigations, with the advantage of better microscopic methods, have confirmed the main results of this early research.

'In his work there was one important particular in which Burdon Sanderson was led astray. He records certain experiments in which, after introducing pieces of thread under the skin of guinea-pigs, tubercular lesions resulted. In the light of present-day knowledge we cannot but doubt that the wounds had accidentally become infected with tubercle bacilli. It is curious to note that the author was not struck by the fact also observed by him that, when the threads had been previously dipped in carbolic acid, no tuberculous lesion resulted, but the effect of the mistake undoubtedly was to keep him from looking for a specific irritant as the cause of the tubercular process.

'Burdon Sanderson's next work (1870, 1872) dealt with the problems raised in the consideration of infection as manifested in the common accident of wound infection, and it is by his researches regarding this condition that he is best known. His first observations here consisted in following up his earlier results with cattle plague and those of Chauveau on vaccine lymph, both of which indicated the non-diffusibility of the infective virus in each of these

cases, and thus pointed to its being albuminous in nature. Arguments were advanced to support the view that, as in cattle plague, the virus in vaccinia was particulate and not a substance in solution. Thus it was pointed out that, while dilution of the lymph caused a diminution of the number of successful inoculations, still, when an inoculation was successful, the effect produced, as judged of by the size of the vesicle and the constitutional disturbance, was as great as when undiluted lymph was employed. It was urged that, if the virus were soluble in water, the effect of diluted lymph might be expected to be uniform success on inoculation, with the production in each case of a mild reaction consequent on the diminution of dose which the dilution would cause. A similar argument was advanced in support of the view that such a virus was non-volatile. If a virus is volatile, how comes it that a person may for long be exposed to infection with smallpox or typhus and only succumb after, it may be, a considerable time? A virus being thus probably particulate and non-volatile, the next problem was as to its nature. It might be either a living substance or it might consist of non-living matter in large molecular aggregates. In Burdon Sanderson's opinion the former view was the more probable, as the enormous increase in the amount of virus which occurs in the body of an infected animal would thus be accounted for. Such a phenomenon appeared inexplicable if a mere chemical substance were concerned in the process. The way was thus opened up for the consideration of what was known of the capacities of the lower forms of plant life. The botanical relationships of the lower fungi to the bacteria found in putrefactive and disease processes were taken up, and experimental evidence was adduced to show the erroneousness of Hallier's view that the latter were only a developmental stage in the life history of the former. It was shown that in external nature, wherever moisture existed, there bacteria were present, but that the living tissues were usually bacteria-free. These preliminary considerations being disposed of, Burdon Sanderson now attacked the main subject of the pathology of blood-poisoning. He pointed out that this type of disease manifested itself in acute septicaemic forms and in chronic pyaemic conditions in which abscess formation was the leading feature, but that the two types passed insensibly into each other. He further pointed out that on anatomical grounds the chronic

pyaemic type could be differentiated from tubercular manifestations. He then showed that in both septicaemia and pyaemia there were invariably present bacteria, especially round forms (these undoubtedly being the pyogenic cocci of modern bacteriology), and that the more intense the inflammation the greater was the number of these present. Though even seven years later (1877) Burdon Sanderson still held the germ theory of disease to be not yet proved, there can be little doubt—though his views are expressed with great caution—that from this time on he regarded the bacteria he had thus described as the causal agents in septicaemia. At any rate he notes that the difference between a simple traumatic inflammation and an infective one is the presence of bacteria in the tissues in the latter case. He also made the further important observation that the passage of exudates containing such organisms through a series of animals had the effect of increasing the virulence of the disease process.

'Soon after these researches were completed Physiology began to occupy more and more of Burdon Sanderson's time. Still, in the later seventies, while head of the Brown Institution, he found time for work at pleuro-pneumonia, and with regard to this disease gave grounds for believing that by intravenous inoculation of the virus a protective vaccination could be effected. Again, in working at anthrax in cattle, he showed that the passage of the anthrax bacilli through the guinea-pig lessened their virulence for the ox. It may be said that neither of these far-reaching results, both of which anticipated later work on immunity, ever received the attention they merited. After these researches his contributions to Pathology were chiefly in the form of critical articles. Among these were his papers on Inflammation, in Holme's *System of Surgery* (1883), which for long was a classic; on Fever, in Clifford Allbutt's *System of Medicine* (1896); and his Lectures on Immunity (1896). Up to the end of his life, however, his interest in the subject never flagged, and his lectures as Regius Professor of Medicine invariably dealt with pathological subjects.

'The task of trying to measure the position which Burdon Sanderson occupied among the pathologists of the nineteenth century is no easy one. On the one hand there is the danger of over-estimating his work by reading his tentative explanations of phenomena in the light of later and

completer knowledge, and of seeing anticipations of later discoveries in results the significance of which were not appreciated at the time they were made. On the other hand, there is the danger of his work being minimized by attention being too much concentrated on the apparent absence of outstanding and permanent results, in forgetfulness of the difficulties besetting the pathological pioneer at a time when morbid anatomy was in its infancy, when microscopic technique was undeveloped, and when the problem of infection was attacked without the aid of botanical knowledge of the commonest infective agents. We must remember that Burdon Sanderson began his work within nine years of the publication of Virchow's *Cellular Pathology*, which is the starting-point of the science in its modern form. From this time onward disease processes had to be judged of by the impress they make on the cells of the part affected. But it is only now being recognized that this is an incomplete view of the position. There still remain the questions of how the morbific agent affects cellular life, and how, if recovery is to take place, such life is to be led back into normal lines. And these were the problems with which in reality Burdon Sanderson's work was concerned. At that time the only disease process which by treatment had been modified was smallpox, and this disease was, as we have seen, one of those to which he first directed attention. Yet even now we know nothing either of the causal agent of this disease or of the scientific principles which underlie vaccination. We cannot therefore judge work such as that under consideration merely by its visible results and apart from its relationships to the gradual development of knowledge and doctrine. But there is also a personal element to be noted. It is a question whether intellectual ability of the high order which Burdon Sanderson possessed may not in reality hamper its possessor in the pursuit of great schemes. Such a one sees clearly the road which ought to be followed, but he also sees too clearly its pitfalls. In Burdon Sanderson's case this may account for the attitude which he assumed towards the problems which his work opened up, and for the hesitancy he displayed in following these results up to definite conclusions. His outlook was almost too wide. It was not enough for him that certain bacteria were found associated with certain disease manifestations: he must needs know the vital capacities of such

organisms and how they worked, before assigning to them an etiological relationship to the conditions in which they occurred. Thus in 1877 he refused to range himself as a supporter of the germ theory of disease, and he then freely stated that knowledge was yet too limited to permit of theories being entertained; it was the duty of investigators to go on accumulating facts.

'We have seen that in several cases the great significance of Burdon Sanderson's observations has never been appreciated, but there can be no doubt that in two outstanding instances his researches had a very definite influence on contemporary thought. His work on the infectivity of tuberculosis was the means of shaping opinion in this country and preparing it for the reception of Koch's discoveries when these came. But, above all, his work on septicaemia and pyaemia caused the germ theory to take firm hold of the minds of scientific physicians. Notwithstanding his own attitude of caution, Burdon Sanderson was looked on, along with Lister, as the chief British upholder of this doctrine. This is abundantly manifest if contemporary literature be studied, for it is he who is attacked by its opponents. But, what is of far greater importance and significance, Lister in several of his papers (*Collected Papers*, Oxford, 1909, vol. i, pp. 276, 365; vol. ii, p. 225) expresses his indebtedness to Sanderson's work in the development of his own views. In judging, therefore, of Burdon Sanderson's position as a pathologist we have to recognize him as the investigator who in this country was one of the first to pursue original work in the department of infective disease, who stimulated work among his contemporaries, and who by keeping interest in the subject intense prepared the way for the acceptance of later and fuller researches.

'He did more than this, however. His outstanding intellectual powers kept him for forty years in a position of great influence, and this influence was always utilized for the advance of Pathology and of scientific medicine. The charm of his personality made him accessible to the humblest student, and every promising work had his encouragement and the benefit of his criticism. He thus knew better than any one those who were likely to make contributions to knowledge, and when he felt that opportunity should be given to a worker his whole influence was exercised in opening up the way for the work

being done. It is as the man who did more than any one else to establish a living school of scientific pathology in this country that Burdon Sanderson will be chiefly remembered.'

In the summer of 1870 Sanderson was appointed to the Professorship of Practical Physiology and Histology at University College, London, as successor to Professor Michael Foster; it was four years later, in April, 1874, that, on Professor Sharpey's retirement, he was appointed his successor, the two chairs being merged into one. In order to strengthen the teaching staff, Schäfer, who had been actual teacher of Practical Histology, was made Assistant Professor, and entrusted with the histological part of the work, to which was subsequently added Elementary Physiology.

In 1873 there was published the *Handbook for the Physiological Laboratory*, a work in two volumes, edited by Burdon Sanderson, and consisting of four sections, written respectively by Klein, Burdon Sanderson, Foster, and Lauder Brunton. Its aim was to furnish a guide to experimental work in Physiology, and this aim was admirably fulfilled. The part on Histology was by Klein, and contained numerous original illustrations. The part on Blood, Circulation, Respiration, and Animal Heat was by Burdon Sanderson; that on Muscle and Nerve by Foster; and that on Physiological Chemistry and Digestion and Secretion by Lauder Brunton. There was great need for such a book, particularly in this country, which had for the time fallen far behind Germany and France in Experimental Physiology; and undoubtedly the *Handbook* did a great deal for the development of the subject in England. It will be noted that the parts of Physiology which Burdon Sanderson dealt with were not those with which he was mainly occupied in his subsequent life. But he had given the closest attention to what he wrote about—so close that to the end of his life he remained in living touch with parts of Physiology at which he had not himself worked

for many years, just as he remained in close touch with Experimental Pathology long after he had given up all experimental work in that department.

Burdon Sanderson was connected with University College for a period of thirteen years, from his forty-second to his fifty-fifth year, and these years were perhaps the most important of his life. Practical Physiology had for the first time become recognized as obligatory upon all medical students by the University of London and the Royal College of Surgeons. As a result, larger accommodation was given for class-rooms and laboratories in connexion with that subject. When Burdon Sanderson succeeded to the Jodrell Professorship of Human Physiology (so called because it was endowed by Mr. T. G. Phillips Jodrell) he set himself to the task of reorganizing the teaching. Of practical work comparatively little had hitherto been done by the ordinary student either in England or elsewhere. The laboratory work, which is now so prominent a part of every medical student's course of training, was, so far as Physiology went, of small account. In every Medical School the change in this matter within the past thirty or forty years has been enormous. University College was one of the first Schools of Medicine to institute this side of science teaching in an organized and developed form, and Burdon Sanderson had a leading part in carrying those new conceptions of medical education into effect. Professor Sharpey, Burdon Sanderson's predecessor in the Jodrell chair, was, as the latter constantly affirmed, the one who might be said to be the originator of the new conception in England. He was a man of very marked individuality, and the influence he had upon his pupils (amongst whom might be counted a considerable number of the English physiologists of the day) was very considerable.

Sharpey was a lecturer who influenced others through what he said and the manner in which he said it. Burdon Sanderson had not perhaps his gifts in this direction. Words did not come to him easily, and he was almost over-fastidious

about form; he could not bear to use an expression not altogether appropriate, and it is to few men that the appropriate word comes at once when called for in extempore speech. Those who heard him lecture even on comparatively popular subjects at the Royal Institution or elsewhere, remember the expression of anxiety, almost amounting to pain, upon his countenance as he spoke, and the difficulty they frequently experienced in following him. His remarkable appearance can never be forgotten. The long thin figure, whose motions expressed the man as much almost as the striking face, which once seen could never be forgotten; the head set somewhat forward on the shoulders; the well-cut features, expressive mouth, deep-set blue eyes that so often twinkled with fun; the brown hair, worn somewhat long; and the general air of self-possession, mixed with an extreme sensibility which made life's lights and shadows seem doubly bright or correspondingly dark and gloomy. Life to him was a serious matter, and the years as they passed left their mark imprinted on his face. Each day had its allotted task; the day's work began long before breakfast-time, and it was continued till the evening. The carefully kept diaries record how faithfully plans of study were adhered to; they are simple records of work accomplished as the clock went round, intermingled with the *nil* or *dies non* expressions that seem to indicate vexation at necessary or unnecessary interruptions. Often when he came down to meals the train of thought he had been pursuing seemed to follow him still, so that he had frequently to be recalled from his reverie to the purpose in hand, perhaps after he had committed himself to one of those forgetful observations that formed the foundation for many entertaining tales amongst his students. No one laughed more heartily than he when the *lapsus memoriae* was pointed out.

He lived in London first in 49 Queen Anne Street, and then in 26 Gordon Square, whither he and his wife moved later on in order that he might be near his work at Univer-

sity College. Besides having the collaboration of Professor Schäfer in teaching work, Sanderson had at this time constant and most valuable help in research work from Mr. F. J. M. Page, who was afterwards Lecturer on Chemistry and Hygiene at the London Hospital Medical School, and who died in 1907. Several important researches on Electro-physiology were the joint work of Burdon Sanderson and Page. The latter first came as assistant to Sanderson at the Brown Institution in connexion with experiments on respiratory exchange, some of which Page published later in the *Journal of Physiology*. Page also took an active part in organizing and carrying out the teaching of physiological chemistry at University College. Among others who gave assistance in the teaching work were Drs. A. D. Waller, F. W. Mott, J. Rose Bradford, and Francis Gotch; but besides these valuable assistants Burdon Sanderson gathered round him a large number of men, many of them young, who were attracted by the personality of the teacher and the facilities given for work in the newly organized laboratories. Amongst those who contributed to the *Papers of the Physiological Laboratory of University College* between the years 1874–81 the following names occur: G. F. Dowdeswell, R. Marcus Gunn, William Osler, J. Cossar Ewart, F. J. M. Page, Augustus Waller, F. A. Dixey, Frederick W. Mott, Victor Horsley, A. R. Octavius Sankey, D. James Williams, G. J. Romanes, Carl Rëyher. The papers were edited by Burdon Sanderson and Schäfer, both of whom likewise contributed largely to them.

Professor Schäfer writes regarding these times:

'My recollections of Burdon Sanderson date from 1870, the time of his appointment as Professor of Practical Physiology in University College, London, in succession to Michael Foster, who had been invited to Cambridge as Praelector of Physiology in Trinity College. My friend and fellow student, Newell Martin, who afterwards went to the Johns Hopkins University in Baltimore, had acted as Foster's assistant at University College, and accompanied him to

Cambridge, entering at Christ's College. I was also pressed by Foster to go to Cambridge, but for various reasons decided to remain in London. Not the least of these reasons was the fact that the new Professor had, on Foster's recommendation, requested me to act as his assistant—an honour I esteemed highly.

'It was early in August, 1870, that I received a note from Professor Burdon Sanderson asking me to call upon him at Howland Street. I well remember the occasion. It was a sweltering day, and soon after I had been shown into the front parlour—the laboratory being at the back overlooking the Mews—a tall distinguished-looking man, in his shirt-sleeves, flung open the door and announced himself as the new Professor of Practical Physiology. I was struck, as no one could fail to be, with his appearance—but still more with the kindliness of his manner, and I had no hesitation in making up my mind to accept the appointment that he offered me. I came away from the interview feeling that I was fortunate in the opportunity of working with and under so charming a personality.

'Up to that time Practical Physiology in University College had consisted mainly of Histology and Physiological Chemistry. But Sanderson had determined that Experimental Physiology should occupy a prominent place in the future teaching, and this part of the subject he himself specially looked after. To me he soon delegated the entire teaching of Practical Histology, and he before long introduced Mr. F. J. M. Page to the Department—ostensibly as his private assistant, but really to superintend the reorganization of the teaching of Physiological Chemistry. The demonstration of Physiological Experiments was an important part of the course of Practical Physiology, and this was carried out in a most systematic way by Burdon Sanderson. It was at first part of my duty to aid in preparing and showing these experiments, and I was thereby enabled to gain experience which has since proved invaluable. The course of lectures was published—with illustrations—in one of the medical papers, and was at the time the only course of its kind in the kingdom.

'An important incident in connexion with the revival of the study of Physiology and Pathology in this country was the coming of Dr. E. Klein, of Vienna, who was already, as a young man, eminent as a worker in normal and patho-

logical Histology and Embryology. It was at Burdon Sanderson's instigation that Dr. Klein was induced to come, and accommodation for his work was offered—first at Howland Street and University College, and later at the Brown Institution. Dr. Klein brought over with him and imparted to us all the most approved methods of fixation, staining, and section cutting—in which the Vienna School was then foremost—and we were able as a result to organize the teaching of Histology on a basis which has served, with of course many additions to the superstructure, up to the present day. This result was merely incidental to the main object which Sanderson had in inviting Klein over, which was the furtherance of work in Pathology—especially that of tubercle—a subject to which Sanderson always devoted special attention.

'The laboratory at University College was at first a single room. But to this others were soon added, and eventually a portion of the main corridor was partitioned off and provided with windows : this served to house the Department of Histology. All the rooms were separated from one another—an arrangement replete with inconvenience. When, however, the north wing of the College was built, accommodation of—for that time—a not wholly inadequate character was found for Physiology in the second story. There it remained until recently, although the extent of accommodation had long ceased to be adequate. It has now been removed to an independent building on the south side of the College.

'Professor Sharpey continued for three years after Burdon Sanderson's appointment to give the systematic lectures on Physiology, but retired in 1874, when under the terms of the Jodrell gift the Chairs of Physiology and Practical Physiology became united and Sanderson became the sole occupant. He nominated me Assistant Professor, and allotted to me a definite proportion of the teaching of Physiology as well as the supervision of the Histology Department. Mr. Page continued to look after the Department of Physiological Chemistry, besides giving assistance to Professor Sanderson in his research work, and a series of assistants, some of whom have since become eminent, helped to carry on the active work of the laboratory. Amongst those who carried out investigations in the Department during Professor Sanderson's tenure of the chair the names of Sydney Ringer, G. J. Romanes, Victor Horsley,

F. W. Mott, Patrick Geddes, Francis Gotch, J. Rose Bradford, W. M. Bayliss, A. D. Waller, and P. H. Chapman may be mentioned. The examination of these names serves to indicate what a wide range of influence Burdon Sanderson exerted upon teaching and work in Physiology during the thirteen years he was officially connected with University College—an influence which did not cease with that connexion but was continued until his death. To him and to Michael Foster—in a remoter degree to William Sharpey—is immediately due the revival of physiological investigation in this country. The first sign of this revival was the appearance of the *Handbook for the Physiological Laboratory*, which was edited by Sanderson and written by himself, Michael Foster, Lauder Brunton, and Klein, and which was necessitated by the fact that at the time of its publication there was in no language any Handbook of instruction for the conduct of a course of Practical Physiology. At the present time almost every teacher has such a Handbook for his class, but few even now are as complete as the one which was brought out under Burdon Sanderson's direction.'

It may be worth while, in view of the above, to consider the developments that were taking place in the teaching of Physiology in England at this time. In 1871 Burdon Sanderson gave in *Nature* an account of the opportunities that then existed for the study of Physiology by English and Scottish students, in reply to an introduction to the account given by Dr. Bowditch of the Physiological Laboratory at Leipzig, in which the writer asserted that in England absolutely no physiological laboratory was open to students. Sanderson pointed out that University College had for many years past been open to any one desirous of conducting experimental inquiries in Physiology and Pathology, and that this permission had been largely made use of. In Edinburgh, also, little could be said of want of opportunities, and though the great London Schools were behind Edinburgh, even in them there was prospect of advance before long. 'The Physiological Laboratory at University College now consists of three rooms, one of which, of large size, is devoted to students, one is employed as a place of research

and for the preparation of materials for demonstration, while the third is used for such special purposes as require a separate apartment.' At present, it was remarked, a greater want than that of laboratories was that of *workers* in Physiology—that is of men already drilled in Chemistry and Physics, and prepared to devote a few years of their lives to continuous physiological and pathological research. The movement towards a more practical method of teaching the theory of Medicine was, as Sanderson says, comparatively a new one, and he hoped—a hope that was realized ere long—that in a very few years great progress would be made, even though physiologists in England could not in their equipment compete with the splendid institutions at Leipzig or Breslau.

'A dozen years of good work will place us again side by side with Germany. . . . This country still maintains its superiority over all other European countries in respect of medical and surgical skill, and has cause to be proud of it. But it is to be borne in mind that the men who exercise that skill were for the most part educated at a time when we could also compete with Germany in science. As science advances, its influence on practice, now so difficult to trace, will increase. If we continue to undervalue it, as we have done, shall we not also eventually lose our practical pre-eminence?'

In the following year (1872) Burdon Sanderson again dwelt on the subject of the direction in which efforts should be made to improve the position of Physiology in this country. The one great reason why physiological research was less successfully pursued in England than we could wish for, lay, in Sanderson's opinion, in the general lack of scientific education, needed both to produce scientific workers and to educate public opinion. The close relationship between Medicine and Physiology was, he held, likely to be a permanent one, on the ground that any science is likely to be studied with more earnestness by those who have to practise an art founded upon it than by others. But the so-called 'medical sciences',

Chemistry, Anatomy, and Physiology, had developed too fast for the resources of the schools. Physiology, once the handmaid of Medicine, had become a science quite independent of the art which brought her into existence, and she now claimed closer relationship with experimental sciences like Physics and Chemistry than with her parent art.

Although Burdon Sanderson admitted that so far Germany had done the greater part of the work of Physiology, he looked with confidence to the future, and considered it a most encouraging sign of the times that Trinity College, Cambridge, had taken a step forward in providing a place where physiologists could study. Doubtless one of the most pressing difficulties that then existed—and was likely for some time to exist—was the want of pecuniary resources. But Burdon Sanderson was confident that if public opinion were once interested on behalf of any scientific object, and particularly if the intelligent classes of the community could be shown that the furtherance of abstract science was a matter of vital importance to our national existence, the money would at once be forthcoming. How true these forecasts were, the later history of Physiology as of other kindred sciences has amply proved.

CHAPTER VI

LATER SCIENTIFIC WORK IN LONDON

It was soon after the issue of the *Handbook for the Physiological Laboratory* in 1873 that the crusade against Experimental Physiology, known as the Anti-Vivisection Movement, came to a head. The subject had not been overlooked by the scientific world before this, since in 1870 the British Association appointed a Committee to report as to the conditions under which in their opinion experiments on living animals were justifiable. The movement was characterized by an amount of misrepresentation which was inexplicable, excepting from the fact that from want of knowledge many persons were absolutely misled as to the true extent and nature of the practice. As a result of this agitation, a Royal Commission, under the Presidency of Lord Cardwell, was appointed in June, 1875, to inquire into the subject, and to report as to what measures, if any, should be taken in respect of regulating the practice of subjecting live animals to experiments for scientific purposes. Burdon Sanderson was invited to become a member of this Commission, but declined the request. A Bill which had already been introduced by Dr. Lyon Playfair, and which Sanderson supported in the main, was withdrawn on the appointment of the Commission. The Bill went further than many scientific men thought right in restricting experiments to those for the purpose of new scientific discovery (thus prohibiting any kind of demonstration), and hence it received but a minimum of support from the scientific world. In regard to Dr. Burdon Sanderson's evidence the Commissioners state in their Report that

'the state of things which he would like to see established with reference to physiological research, is such as would

unquestionably discourage the making of experiments by any one, excepting by persons trained in a School of Physiology. He thinks there would be some inconvenience attaching to legislation, but also that there would be, even for Physiology, some advantages. The difficulties would apply with reference to private individuals, but though he thinks it would be an objection if private individuals should be interfered with, he does not lay great stress upon that, because they are few and will probably become fewer year by year. As research is carried on into the more difficult parts of Physiology, the investigator requires appliances of greater complexity which are exceedingly expensive, and even if he could afford to try them, he would have to build a place adapted for their use.'

The evidence given by the English physiologists was eminently satisfactory, inasmuch as it bore out the fact that they were careful to inflict as little pain on animals as might be. But certain evidence given pointed in the minds of many to the necessity of legal restrictions being made. The Commission reported in January, 1876, and a few months afterwards Lord Carnarvon introduced a Bill to the House of Lords entitled 'An Act to amend the Law relating to Cruelty to Animals'. This Bill, which was carried into law, went considerably beyond the recommendations of the Royal Commission. In reference to it a Memorandum was issued by sixteen eminent Professors and Lecturers on Physiology, amongst whom Burdon Sanderson's name appears, pointing out certain important considerations to be kept in view. The concluding sentences sufficiently indicate their manner of regarding the case:

'It is on the scientific investigator himself that the responsibility must ultimately rest of determining what is the best method of accomplishing a given scientific result, and by what means the *greatest possible result may be obtained at the least possible cost of suffering.* If restrictions are supposed to be necessary to control the conduct of careless individuals, let them be imposed: but so long as scientific men exercise their responsibility in the humane spirit which has hitherto guided investigators in this

country, they have a right to ask that no unnecessary obstacles should be placed in the way of studies which are peculiarly laborious, unremunerative, and difficult.'

In connexion with the Vivisection controversy Burdon Sanderson was brought into close relationship with Darwin, who was, if possible, even more sensitive to any unnecessary infliction of pain on an animal or helpless creature than Burdon Sanderson himself. They united together in regard to Sir Lyon Playfair's Bill and a proposed petition, which was dropped, in its favour. As regards that Bill Darwin writes, 'I am delighted to hear that all your exertions and labour will in all probability be rewarded by success. I think every one who has in any way aided has done good work in the cause of Humanity and Science.' To those who were personally acquainted with Burdon Sanderson it seems incredible that he should have been regarded as capable of wanton cruelty, but the letters he for many years received—many of them from ladies—were inconceivably brutal in tone, and to one of his nature the sense that such things were said of him was very painful. He puts his own views very clearly and simply in a letter to a lady of his acquaintance who had written to him on this subject:

'You will find reliable and capable information on the subject to which your letter relates in the Report of the Royal Commission presided over by Lord Cardwell. Although a Blue-book is not generally very attractive reading, you will in this case not find it very laborious to read enough of it to acquire a correct notion of its purpose and of the facts on which the conclusions are founded.

'You will see from that Report that the statements which have been of late industriously circulated by agitators are without foundation, that all scientific investigations in this country are carried on by persons of known position and responsibility, and that the number of men engaged on such investigations is exceedingly small (not more than twenty), as compared with the number occupied in other pursuits of infinitely less importance.

'The agitation which is at present going on on this sub-

ject is unfortunately conducted by persons with whom deliberate falsehood seems to be a habit, and who do not scruple to use any means that they think most likely to be successful to accomplish their ends.

'The principles on which I have always acted myself, and on which all physiologists act in this country, are these: We endeavour in using animals in scientific investigations to accomplish as much good as we can at the expense of as little suffering as possible. Let me remind you that this is *not* the case in general as regards the treatment of animals for sport, food, or luxury.

'For sport, hundreds of foxes are subject weekly to tortures which are absolutely needless and wanton.

'For food, animals are daily subjected to painful operations without anaesthetics.

For luxury, animals are subjected to agony and meet painful deaths in traps.

'Compare these mountains of suffering, most of which is needless, and to mitigate which these wretched scribblers would not raise a little finger, with the very trifling and almost always unavoidable suffering which we unwillingly inflict for the most important purposes, and then see whether you cannot spare us, who have during the last twelve months or more had our lives made miserable to the best of an enemy's ability, a little sympathy.'

Mr. George J. Romanes wrote a letter, accompanying a snared rabbit of revolting appearance, which he suggests should be shown to the Royal Commissioners to demonstrate the real cruelties that went unnoticed. He describes the agonies inflicted by snares set in the evening on rabbits which have to endure many hours of torture before they are released or strangled, and then goes on to speak of the pain of trapping, which he describes as being as 'extreme as pain can be'.

'Indeed I doubt,' he says, 'whether it be possible to devise a mode of torture by which a greater amount of pain could be inflicted on an animal. Pain of any greater severity would be incompatible with any considerable duration of life; but a spring trap, by exerting its unremitting pressure upon the inflamed portion of the leg it has cut and broken, keeps up a constant pain of great severity,

but yet not enough to kill the animal by nervous shock.... If I were asked to state the most revolting thing in the way of cruelty that I have seen, I should not hesitate to say, 'A rabbit warren by moonlight during the trapping season'; but any description I could give would not convey an adequate picture of such a mass of struggling agony."

Certainly the difference between the magnitude of such abuses, which take place in thousands of cases all through the country, and those, if any, which might take place in the laboratory, and which at least have some good end in view, is, as Mr. Romanes says, very striking.

The controversy proceeded with unabated vigour. In 1881 Mr. Romanes defended Dr. Sanderson in *The Times* against accusations made against him by Miss Cobbe. Of this letter Darwin writes to Mr. Romanes: 'I write to say how I, and indeed all of us in the house, have admired your letter in *The Times*. It was simple and direct. I was particularly glad about Burdon Sanderson, of whom I have been for several years a great admirer.'[1] This was not very long before, as we shall see, the question was once more raised in Oxford in an acute form.

The foundation of the Physiological Society, was one outcome of the Anti-Vivisection Movement. On March 31, 1876, a meeting of physiologists was held at Burdon Sanderson's house, 49 Queen Anne Street, in reference to which there is a draft letter to Lord Cardwell in his diary. He says in it:

'This evening a meeting took place in my house at which eighteen gentlemen were present, the number including nearly every one who is actually engaged in physiological research in England. The purpose of the meeting was to consider the desirableness of establishing a Society for the advancement of the science, but at the close of the meeting the subject of legislation was discussed and an expression of opinion was unanimously agreed to, to the effect that if any legislation were to take place no objection would be offered by any of those present, provided that it were in accordance with, and did not go beyond,

[1] Darwin's *Life and Letters*, vol. iii, p. 208.

the recommendations of the Royal Commission. Although it is not contemplated to take any steps in consequence of our deliberation, it was thought that it might not be without utility to make your lordship aware of the conclusions arrived at.'

At this meeting it was decided to establish the Physiological Society, the meetings being also dinners. The first regular meeting was held on May 26 of the same year, when Darwin was elected an honorary member, a mark of appreciation which gave him much pleasure. It soon became the custom to have an informal scientific meeting for discussion of verbal communications and experimental demonstrations before each dinner meeting, and to hold these informal meetings at different physiological laboratories in rotation. The Society has never had a President or a regular place of meeting, and for many years it published no *Proceedings*, although recently this custom has been relaxed, and short abstracts of many of the papers are printed. The Physiological Society has steadily grown in membership and importance since its foundation, and probably few scientific societies have fulfilled their purpose so effectively. The programme is invariably a full one and the attendance large. The members now number 268. Most of the meetings are in London, but a certain proportion are also held in Oxford, Cambridge, and other University towns in England and Scotland.

The active support of Burdon Sanderson, Foster, and the younger men whom they were influencing, made the Society from its outset a living force for the advancement of scientific knowledge. Much of its success has, however, also been due to the establishment by Foster of the *Journal of Physiology*, in which appear fully the more formal accounts of most of the experimental work first described and discussed at meetings of the Physiological Society. Each member now receives the parts of the *Journal* as they appear, and the *Proceedings* of the Society, containing such abstracts or short communications

as it may be desired to publish at once, are printed in the *Journal*. Several other scientific societies in this country and America have been formed on the model of the Physiological Society. On account of the great development of Natural Science, and its constantly increasing specialization, many of the functions which were formerly fulfilled by the Royal Society have naturally become delegated to special societies such as the Physiological. Something is doubtless lost in this way; but there can be no doubt that the gain is enormously greater than the loss.

In 1873 Darwin was much occupied with the study of what he calls his 'beloved Drosera: a wonderful plant or, rather, a most sagacious animal'. In connexion with this work he was brought into communication with Burdon Sanderson; he was, as he says, 'struck with the notion that the excitable cells of the plant might exhibit phenomena in all essentials similar to those present in excitable animal tissues.' Burdon Sanderson entered upon the line of study suggested with immense interest and zeal, and this was indeed to prove the basis of the work that was to occupy him for the rest of his life. We find in his diary constant references to his communications with Darwin on the subject, and on September 12, 1873, his results were deemed so satisfactory as to be worthy of being telegraphed to Darwin's country house. In Mr. Darwin's many letters to Burdon Sanderson he expresses himself as deeply grateful for the assistance given him in his work.[1] Professor Gotch gives an admirable account of the work undertaken by Sanderson in this regard, as well as of other work carried on at the same time, in his Obituary Notice written for the Royal Society, and from it we may quote the following passage:

'Drosera led to Dionaea, which is characterized by the rapid character of the movement of its leaf lobes when certain hairs upon their inner surface are touched. The exquisite sensibility of the hairs to mechanical displacement

[1] *Life and Letters*, vol. iii, p. 822.

and the extraordinarily rapid character of the mechanical alteration in the leaf attracted the attention of Burdon Sanderson, who saw in this plant an organ admirably fitted for the investigation of fundamental phenomena.

'He therefore, about 1875, commenced an investigation as to the changes which were associated with this active process and the conditions which influenced their production and character. In the course of this inquiry he discovered that there was a pronounced electromotive change in the leaf every time it passed into the active state. He then devised an ingenious method for keeping the leaves forcibly open by fixation in plaster of Paris, and demonstrated that although no mechanical movement could now take place when an excitable hair was touched, yet each local stimulus of this kind still evoked an excitatory response, since the same active electromotive change was observed. This active "electrical response" was of considerable magnitude, although of comparatively short duration, and its peculiar interest lay in the circumstance that it was successively developed in all parts of the leaf lobes, indicating the propagation of some active cellular change from the seat of the stimulus to more remote regions.

'In 1877 an account of this discovery, together with the experimental determination of the time relations of this electrical response, formed the subject of the Croonian Lecture of that year, the title of the lecture being "The Mechanical Effects and the Electrical Disturbance Consequent on Excitation of the leaf of *Dionaea muscipula*." Five years later, in 1882, a more extended and detailed account of the phenomena was published in the *Philosophical Transactions*, entitled "The Electromotive Properties of the Leaf of Dionaea in the Excited and Unexcited States". From the careful analysis which he had made of his experimental observations, he concluded that the excitatory disturbance, "by the mode of its origin, the suddenness of its incidence and the rapidity of its propagation, is distinguished from every other phenomenon except the corresponding process in the excitable tissues of animals." "In the one case as in the other", he continues, "we must regard the electrical change as a visible sign of an unknown molecular process." The actual mechanical displacement he agreed with Pfeffer in considering as probably related to "the diminution of the turgor or water-charge of the protoplasm of the excitable cells". The experimental

evidence which he now brought forward appeared to indicate two possible sources of electromotive change ; there was shown to be an initial or primary change, sudden in its development, brief in its duration, and always of the same general type ; this he regarded as the electrolytic indication of the peculiar molecular alteration in the plant cells which constitutes the excitatory state ; it was succeeded by a second change of longer duration and often of different sign, which was attended by a prolonged residuum or after effect ; this he regarded as associated with diminished turgor and consequent displacement of water. It was only by accurate methods of recording that he was enabled to discriminate between the primary and secondary effects and to show that as regards the primary or initial change the active excitatory state is fundamentally the same, whether it occurs in these vegetable cells or in excitable animal tissues.

'A third paper upon the electromotive properties of the leaf of Dionaea appeared in the *Philosophical Transactions* of 1888. This paper included a number of experimental observations as to the behaviour of what he termed "modified leaves"; the observations were made in Oxford, and involved the use of a special double rheotome ; the capillary electrometer was also employed, and photographic records of the excitatory electrical changes are given in the paper.

'The observations show several interesting facts, but the chief points brought out refer to the so-called "modification", which is produced in a leaf when it is subjected to the flow of even a weak voltaic current. The modification reveals itself as a permanent alteration in the amount, and even the sign of the electrical state of the inactive leaf-surface. It is localized to the part which the modifying current has traversed, and is associated with a remarkable diminution in the high electrical resistance of the tissue, this diminution being strictly confined to the modified region. Finally, the records give demonstrative proof that, when any such modified area is thrown into the active state, either by direct stimulation, or by the arrival of a propagated excitatory wave aroused elsewhere, then the electrical response in this area, whilst it has the same time relations, may be entirely changed as regards its sign. This extraordinary reversal is related to the sign of the inactive tissue, and, when through a modifying agency this

is altered, then the sign of the active change may be similarly reversed.

'From the constancy of this relationship, Burdon Sanderson inferred "that the constantly operative electromotive forces, which find their expression in the persistent difference of potential between the opposite (leaf) surfaces, and those more transitory ones which are called into existence by stimulation, have the same seat, the opposition between them being in accordance with the general principle that, whereas the property which renders a structure capable of undergoing the excitatory change, is expressed by *relative positivity*, the condition of discharge is expressed by *relative negativity*."

'Between the first and second papers on Dionaea he had, whilst at University College, London, pursued the same method of inquiry in the cardiac muscular tissue of the frog. His investigations upon this subject are set forth in a paper published in 1880 in the *Journal of Physiology*. These researches have been taken as a model for most subsequent work of this character, whatever the tissue which has been utilized for such electro-physiological investigations. The paper is entitled "The Time-relations of the Excitatory Process in the Ventricle of the Heart of the Frog", and was followed three years later by one "On the Electrical Phenomena of the Excitatory Process in the Heart of the Frog and of the Tortoise, as Investigated Photographically." In both these investigations he had the assistance of Mr. F. J. M. Page. The whole research has been long regarded as classical through the exactitude of the methods of observation, the rigorous determination of the influence of various surroundings or accessory factors, and the lucid interpretation of the phenomena. It conclusively demonstrated that the diphasic effect (or change of electrical sign) which is observed when two contacts are placed on the uninjured surface of the frog's heart, is the algebraic sum of two monophasic electromotive changes each of similar sign but developed successively, one under each of these two contacts. Hence it is not an indication of the occurrence of opposite types of tissue change, but is simply the expression of a similar type of monophasic change occurring at different times in different localities. Finally, he demonstrated that the reason why the change begins under one contact before it occurs under another one, is solely because the excitatory state like the

muscular contraction is propagated in the differentiated cells which compose the cardiac tissue. For these researches he was, in 1883, awarded a Royal Medal.'

The year 1876 was darkened by a serious family disaster, which occurred under very distressing circumstances. On the 21st of January, in the midst of a blinding snowstorm, Dr. Burdon Sanderson's elder brother, and his wife and family, were travelling from their home in Northumberland to London on their way to Rome, where they intended to pass the spring. At Abbotts Ripton, near Huntingdon, the train ran into some wagons in process of being shunted, and a few minutes later a down train from Leeds dashed into the wreck of the other train, killing many who might otherwise have escaped. Mr. Richard Burdon Sanderson's two and only daughters were killed, his wife was much hurt, and he himself only survived three months. Professor Burdon Sanderson at once went to Huntingdon, taking with him an eminent surgeon, and remained there for a week: indeed, until his brother could be brought to his own home in London, he was constantly with the patients. On April 30th his brother died. Richard Burdon Sanderson was the first promoter of the 'Conciliation Board' of coal-owners and colliers at Newcastle-on-Tyne[1], and was also the originator of the first Reformatory for boys in Northumberland. He was a man of marked ability who had done much for the welfare of Newcastle-on-Tyne and the county in which he lived, and his death was a severe public loss as well as an acute personal grief to those connected with him.

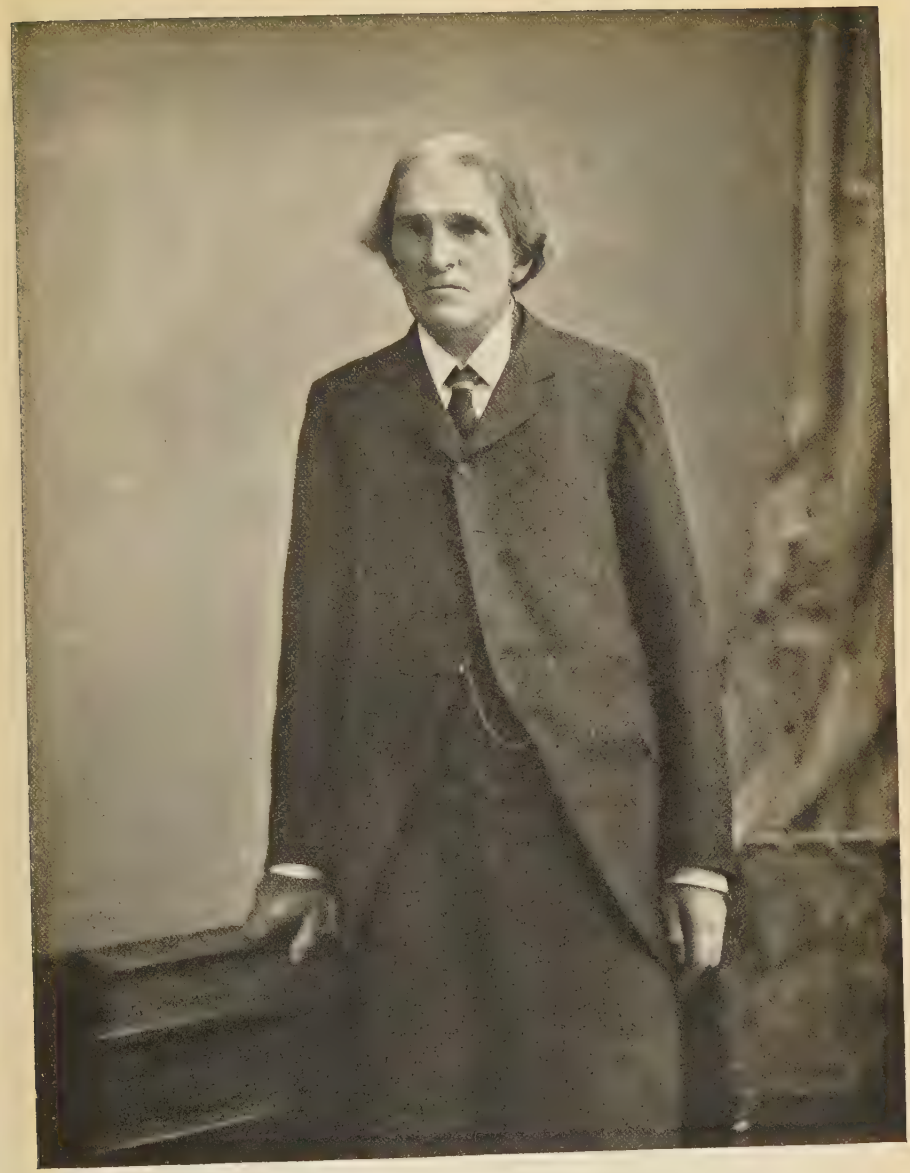
In 1881 a Royal Commission was appointed to inquire respecting smallpox and fever hospitals in the metropolis, under the presidency of Lord Blachford, and on it Dr. Burdon Sanderson served. This Commission had the very important task of advising as to the steps which should be taken in regard to outbreaks of fever such as the severe

[1] The Durham Joint Committee was probably the first board of employers and workmen established for the conciliatory settlement of disputes, and Mr. Burdon Sanderson was its chairman until the date of his death.

outbreak of smallpox in 1871, and the mode which should be adopted of dealing with the objections taken by neighbouring proprietors to the establishment of hospitals for infectious diseases in their midst, difficulties which had occasioned certain hospitals to be closed. When we read the report and discover the results which ensued from the non-existence of the compulsory notification and isolation of fever cases, it seems almost incredible that such a state of matters should so recently have existed in our country. The Commission made important recommendations in this regard, and also recommended that the distinction of pauperism should be abolished in the Metropolitan Asylum Boards' Hospitals. They considered that hospitals for smallpox and other infectious diseases must continue to exist in London for severe cases of illness, although the number of cases dealt with might be materially reduced by removing the less acute and convalescent cases to the country, and they made recommendations as to the structure and government of the city hospitals. In this matter Burdon Sanderson took a special interest, and he himself drew up a scheme of hospital construction and mechanical ventilation which he believed would reduce to a minimum the danger of infection in the neighbourhood.

Another matter of interest about this same time was the institution of the Research Scholarships of the Grocers' Company. In 1882 Mr. Kingdon, F.R.C.S., afterwards Master of the Grocers' Company, consulted Sir J. Simon, in his capacity of medical adviser to the Government, as to the steps which the Company might take for the promotion of sanitary science. Simon suggested that advice would be desirable, and suggested the names of Professor Tyndall, Professor Burdon Sanderson, and Dr. (afterwards Sir George) Buchanan as his colleagues. These four were made a body of Assessors to advise the Court, and the procedure advised by them was to found three regulated scholarships of £250 a year, open to subjects of the United Kingdom, and a quadrennial Discovery Prize of £1,000

open to the whole world. To the regulation and adjudication of these scientific scholarships there are frequent references in Burdon Sanderson's papers. The institution by the Grocers' Company of substantial scholarships for the encouragement of scientific research was a new departure which has proved eminently successful and remarkably fruitful as regards the work carried out by holders of the scholarships; and several other endowments on similar lines have since then been established in this country.



J. Burdon Sanderson

CAL.

From a photograph by Bassano

CHAPTER VII

REMOVAL TO OXFORD

1882—1884

In the year 1882 certain negotiations which had been carried on for some time came to a head in the appointment of Burdon Sanderson as Waynflete Professor of Physiology in the University of Oxford. This chair was one of the new foundations instituted by the University Commissioners of 1877, for the support of which the funds of Magdalen College were rendered responsible. The prime mover in the proposal for asking Burdon Sanderson to come to Oxford was Henry Acland, Regius Professor of Medicine, with whom he had lately been brought into connexion through his occasional journeys to Oxford for examination purposes, and reciprocal visits on Acland's part to London. Acland was desirous of bringing to Oxford one who would give distinction to the office of teacher of Physiology, and who would make that office one of real importance in the University. After considerable delay the appointment was made in November, 1882, and Burdon Sanderson at once went to the scene of his future labours to make necessary arrangements. On December 12th he was elected Fellow of Magdalen, and a few months later he was settled in his new home in Banbury Road. The following letter to Professor Acland explains on what grounds Burdon Sanderson accepted his new office:

'June 20th, 1882.

'My dear Acland,

'Your note of yesterday calls upon me to decide one of those very important questions which present themselves only at long intervals in one's life. Am I to begin

a new chapter, which for me will be the last, or not? If I had not already considered the question I could scarcely answer so soon. As it is there is no reason for delay.

'If the appointment is offered me I will at once accept, provided that the conditions are such as I assume them to be; but I do not intend to engage in a contest.

'Of these conditions the only essential one is the assurance that sufficient funds will be forthcoming for the installation and maintenance of a museum and laboratory. On that subject I have no information. It would be clearly unwise for me to accept an appointment without the certainty that these funds will be provided.

'From my own experience I know that the necessary plant for this department of teaching would cost about £2,500. As regards the building, all depends on its position and the architectural conditions which have to be fulfilled by it. A temporary annexe to the Museum could be erected for a small sum. For maintenance and menial service, the annual sum required would be about £250.

'I have now a fairly complete laboratory for teaching, and private rooms for original work, which are well arranged for the purpose. Whatever attractions Oxford might offer, I could not give up the opportunity of continuing that work. It is therefore clear that it would be inexpedient for me to move, unless I can be assured that the means provided will be sufficient to save me from all necessity of expending money on the material and appliances for teaching, excepting the payment of a teaching assistant, and that the provision for those purposes should be virtually if not actually made beforehand. For I could not come to Oxford with the prospect of having to engage in a contest about funds.

'I trust you will not consider this letter unreasonably long, considering the importance of the subject. Many thanks for the cordial words contained in your note.

'Very truly yours,

'J Burdon Sanderson.'

Although the change had been in discussion for some time, and though after due consideration Burdon Sanderson had made up his mind to the step, it was not without regret that he left London and the work at University College, which had given him so much interest and pleasure,

for a life which was unknown, and where certain difficulties seemed to threaten. It was not long indeed before these difficulties began—difficulties which are well described in the *Life of Sir Henry Acland.* The accommodation and equipment in the Museum was totally inadequate for the teaching of Physiology, and the Commissioners 'had made no provision for "plant" and appliances, and had forbidden any allocation of the income of the Professorship in this direction.' It was necessary, therefore, that the University should supply the deficiency out of the Common Fund. In February, 1883, Convocation voted without demur the sum of £1,500 for instruments and apparatus for the new Professor, and it was intimated that a much larger sum would be required for building purposes. On May 29th notice was given of the intention to ask the Curator of the University Chest 'to expend a sum not exceeding £10,000 in the erection of a Laboratory, Working Rooms, and Lecture Room for the Waynflete Professor of Physiology, and in providing fixtures, warming apparatus, and gas for the same.'[1]

The storm that broke out is only intelligible when we reflect on the various forces that were at work in the University. There were, to begin with, those who simply thought the expenditure extravagant; then there was the easily comprehensible dislike of the Conservative element in the University, represented by those who formerly opposed the construction of the Museum, to the development of the scientific side. But whatever might be the ostensible grounds for the opposition, the most serious objectors to the grant were those who pointed to the fact that the newly appointed Professor held a licence for research experiments under the Act of 1871 known as the Vivisection Act, and had played a prominent part in the controversy on the subject, besides being Editor of the *Handbook for the Physiological Laboratory.*[2]

[1] *Life of Sir Henry Acland*, by J. B. Atlay, p. 421.

[2] Mr. Nicholson, of the Bodleian Library, stated in a circular that

'When the decree was submitted [to Convocation] on June 5th, it met with a strenuous opposition on scientific, financial, and humanitarian grounds, and was only carried by a majority of three in a house of 173, 88 placets and 85 non-placets. The late Warden of New College (Dr. Sewell) insisted that while the University was largely in debt it must learn to do without costly luxuries, and the Warden of Keble (Dr. Talbot, now Bishop of Southwark) expressed himself as not satisfied with the propriety of the vote, though most reluctant to countenance any opposition to scientific studies.'[1]

The assistance here given by Henry Acland was most valuable. His acknowledged moderation and the respect borne to him in the University were guarantees that no needless experiments would be performed; and as a 'certifier' under the Act he was known to take the greatest trouble in the performance of his duties. 'He reminded Convocation that "vivisection played only a small part in physiological inquiry", and that the latter subject was of the utmost importance in relation to the study of medicine, neither of which points of view had hitherto received their due attention.'[2] Burdon Sanderson's argument was that though of course no experiments involving vivisection would ever be made by students, he could not bind himself not to make use of animals in his private investigations, although he would continue to do his utmost to limit the amount of suffering endured as far as it was possible to do so. The laws of secretion, respiration, circulation, and other physiological processes in the human being had all been learnt by observation on living animals. So also had the laws of spread of infectious diseases.[3]

This, however, proved to be but the beginning of the great controversy. The anti-vivisection party, seeing how

permission would be given to Dr. Burdon Sanderson to use the building for 'all kinds of physiological research and illustration, which I and multitudes of other people hold to be immoral'.

[1] *Life of Sir Henry Acland*, pp. 421, 422

[2] *Ibid*, pp. 422, 423.

[3] *Times*, June 6, 1883.

near they had come to victory, had a memorial drawn up and submitted to the Hebdomadal Council, praying 'That, without further order of the University, buildings and appliances provided by the University be not used for the performance or exhibition of experiments involving pain to animals, or of any operation on domestic animals.' The petition accompanying the memorial was signed by one hundred resident members of Convocation, including not a few names of weight, and by some fifty non-residents[1], but this did not prevent the rejection of the memorial.

The battle was now renewed with increased vigour, and the bitterness of the so-called anti-vivisection party was extreme. Letters, anonymous and otherwise, and disgraceful to their writers, were once more written, calling down every sort of retribution on the so-called torturers, both in this world and the next. The University party soon found an opportunity for bringing up the question in Convocation once more.

'It had been decided to raise the £10,000 by a sale of stock, representing moneys derived from the sale of land formerly held by the University in trust from the Bodleian. To do this an application was necessary to the Land Commissioners, in whose name the proceeds of the sale were standing, and a decree of Convocation was required for the purpose. Regarding the matter as settled once for all by the vote of the previous June, the promoters of the scheme had set the decree down for February 5, 1884, and had no anxiety as to the result. At the last moment it came to their knowledge that a strong whip was being circulated among the non-resident members of Convocation, calling on them to come up and *non-placet* the decree. The appeal emanated from a "Committee of resident members of Convocation", whose declared object was "to prevent the establishment out of University funds of a centre of vivisection in Oxford". It was headed *Vivisection in Oxford*, and took the form of a pamphlet of ten closely-printed pages, swelled by a liberal selection of extracts relating to Dr. Sanderson from the Report of the Royal Commission.'

The object of the opposition was no less than to drive

[1] *Life of Sir Henry Acland*, p. 423.

from the University the holder of the Chair of Physiology, and the methods adopted were of a kind which it was difficult to justify, even were the object as desirable a one as its promoters avowed. These did not, however, count on the calmness and dignity with which these attacks were received—a dignity which was more effective than would have been violent recrimination.[1] Dr. Acland wrote to *The Times* explaining the circumstances of the case, and in view of the personal canvass that was going on of non-resident members of Convocation, he appealed to members who wished to defeat an object which would be so prejudicial to the interests of education in the University, as well as unjust to the distinguished Professor in question, to disclaim so unusual a proceeding as that suggested.

On the day appointed the Sheldonian Theatre was crowded with supporters of both parties. The Dean of Christ Church commenced by expressing his absolute confidence in the kindness and humanity of Professor Sanderson, who would be the last man to inflict needless pain, and protested vigorously against the way in which the opposition had been organized without notice given. Professor Freeman then arose in his most vehement mood. 'What was science?' he asked. The historian was as much a man of science as the man who operated on live rabbits. He added, in regard to the practice of vivisection, that the historian might equally claim to illustrate the siege of Jerusalem by reproducing its horrors, or the festivities at Kenilworth by a bull-baiting. A vote of £10,000 would not, he maintained, have been carried by the School of Athens. He concluded amongst uproar that strange things were being done in Oxford. This showed the character of the speaking in reference to what was called the establishment in Oxford of a 'Chamber of Horrors'. The Warden

[1] Sir William Church wrote that 'he would never forget his [Burdon Sanderson's] striking face and figure as he stood quietly leaning against the wall of the Sheldonian Theatre during the momentous debate in Convocation on February 5, 1884.'

of Keble supported the vote, and Acland created a favourable impression by an able speech. Much tumult followed, especially during the Bodley Librarian's (Mr. Nicholson's) speech, and then the vote was taken and carried by a majority of 41—188 placet and 147 non-placets. *The Times* newspaper commented strongly on the debate in view of the fact that Parliament, on the recommendation of one of the strongest Royal Commissions ever appointed, had legislated on the matter, and that physiological experiment was under the severest limitations consistent with any kind of progress in discovery: 'Men like Dr. Sanderson are not only humane, but they are conscious that public opinion is awake on the matter, and their discretion as to what should be done or what should not is absolutely to be trusted. It is to be hoped that the sensible action of Convocation will not only encourage the Waynflete Professor to proceed as his scientific conscience may guide him, but will convince the well-meaning but irrational opponents of scientific freedom that further action on their part would be not only vexatious but unsuccessful.'

An amusing incident in the debate was a reference to a certain experiment supposed to be painful, in regard to which Burdon Sanderson was reported to have said, 'This beautifully simple experiment . . . I had the pleasure of repeating very frequently.' It appeared after much discussion, and after Sanderson himself had been asked to explain, that the passage had been misquoted and that the animal was dead, whereupon an apology was tendered to him by the speaker.

This was not, however, the end, for in the following March the question came again before Convocation on the demand for the annual grant of £500 for coal, gas, and water. A memorial was drawn up calling on members to refuse to sanction 'the performance of physiological experiments on living animals, commonly but inadequately described as vivisection', and demanding whether Oxford

will allow it to be said that she sanctions the doctrine of the physiologist that knowledge may justly be acquired at the cost of torturing God's creatures. This memorial was signed by many distinguished University men, and amongst others by Ruskin and Charles Dodgson ('Lewis Carroll'). It was argued that though Burdon Sanderson might not use such experiments for purposes of demonstration his successors would be free to act differently. There was of course a counter-memorial, equally or more influentially signed.

On March 10, 1884, the Sheldonian was again crowded, and Jowett presided as Vice-Chancellor. Again there was heated speech and much noisy demonstration. In fact the noise of shouting made some of the speeches inaudible. The vote was this time conclusive—412 for, 244 against. And this brought this bitter controversy, so far as public and official protest went, to an end. The columns of *The Times* show, nevertheless, how keenly the Vivisection controversy was kept before the public during the ensuing years. The venom displayed by private persons is almost incredible. Postcards and letters poured in, sometimes addressed to the 'damned and detestable Vivisector', calling down upon the recipient every sort of evil. And this was to a man to whom the ordinary cruelties of the dinner table, the boiled lobster and carelessly shot game, were sources of disgust—disgust which apparently never suggested itself to his detractors. Fortunately the friends who knew him best, and they were many, were loyal and true, and their wholehearted support carried him through this troublous time.

As soon as Burdon Sanderson was fairly established in Oxford his energies were naturally directed to the development of the Medical School, the foundation of which had been in great measure the work of Henry Acland. Acland had indeed laid the foundations and fought the first battles for the cause, but much remained to be done to make that School a reality. The building of the Physiological

Laboratory, which Convocation by its vote of June 5, 1883, resolved to erect, was completed about three years later. There was still, however, no provision for the teaching of Human Anatomy to students of Medicine. To remedy this defect a Lectureship in Human Anatomy was instituted, and a small temporary building for teaching was erected in the yard of the Physiological Laboratory, the money being raised by private subscription. Dr. Arthur Thomson, of Edinburgh, was appointed lecturer, and under his energetic direction the teaching of Human Anatomy was soon placed on a firm foundation. A few years later a proper building was provided and Dr. Thomson was appointed Professor. A statute, whose effect was to constitute a new Faculty of Medicine in the University, was passed in 1886.

The opponents of the new departure in medical teaching urged that Schools of Medicine in London and elsewhere possessed opportunities for practical instruction in Medicine, Surgery, and Midwifery which were not to be had in Oxford, and that it would be better that these schools should also teach Human Anatomy, Physiology, and Pathology, in so far as the latter subjects form a part of professional education. They argued that in Oxford principles only should be taught, and not their application. The opposite view was that although the great hospitals must offer paramount advantages for the acquirement of practical knowledge and skill, better means existed, or would exist, in Oxford for teaching the preliminary scientific subjects in the medical curriculum than would exist elsewhere.

Burdon Sanderson's opinion was that the changes that had come about—the new degree in Surgery, and the introduction of Pathology, the study of the nature and causes of disease, and Organic Chemistry in its application to Medicine, as new subjects of examination—meant a new departure in the teaching of Medicine in Oxford. It meant that Oxford in the future intended to take up the task of medical instruction as all other Universities in the United

Kingdom had already done, and to the newly constructed Medical Board he looked as the means for accomplishing this object by the control it would exercise over the examinations, and the regulations it would prescribe. Oxford should, in Burdon Sanderson's view, be a place where all the branches of medical knowledge should be taught which come within the range of University studies, in such a way as to meet the requirements of the medical student, and that with this view a beginning had been made in the new department of Physiology and Human Anatomy. The student must, however, also have instruction in Chemistry as applied to Medicine, and acquaint himself with the superficial aspects of disease and the means in common use for relieving it, by attendance at the Radcliffe Infirmary, under the direction of the medical and surgical staff. But all this involved organization of a very definite sort.

To Burdon Sanderson medical study divided itself into what might be called Laboratory study and Ward study. In the former he believed that Oxford had advantages which none of the schools attached to the great London hospitals could approach. For the latter only a certain preparatory drill could be given in Oxford, since to learn the practice of his profession every man must occupy his last two or three years in London,or some centre of population where his opportunities would be less limited. The business of the University training at Oxford was to prepare him to make the best use of his opportunities. A difficulty which lay in the way of medical study in Oxford—the non-recognition by the licensing bodies of Oxford teaching—was removed in 1885. The 'General Council of Medical Education and Registration' then accepted Oxford as a 'recognized' medical school, the students in which might register themselves as 'students' under the Act. From this time onwards the organization of medical work in Oxford has proceeded apace.

A serious break in Burdon Sanderson's work occurred in

1884. He had recently taken to tricycling, and in descending Farly hill, near Basingstoke, he was upset and seriously hurt on June 14th. He was carried to Basingstoke in a dog-cart, and lay there until June 30th, when he was removed to Oxford. But he had a dangerous attack as a secondary result of the accident soon after his arrival, and it was not until August 15th that he was able to be moved downstairs. It was a long time indeed before he regained his strength, and one which was specially irksome, occurring as it did at a period when he was much occupied with the new buildings and their equipment.

In the latter part of this same year, Burdon Sanderson began to sit to Mr. Ouless for his portrait; the first portrait did not satisfy the artist, and a second was painted in the following year. Sanderson also gave sittings about the same period to Mr. Hope Pinker for the very excellent marble bust which is now in the Museum at Oxford. In 1893 Mr. John Collier painted Burdon Sanderson as at work in his Laboratory. This picture is now in the Lecture Room of the Physiological Laboratory at Oxford. His portrait also appears in Holman Hunt's well-known picture of the scene on May Day Morning when a song is sung from the top of Magdalen Tower. A successful portrait, now in Magdalen College Hall, was likewise painted by the late Mr. Furse. Of cartoons there was an excellent one by 'Spy', published in *Vanity Fair* in 1894, and F. C. Gould made a good one of the 'Great Medicine Man' in the *Westminster Gazette* at the time of Burdon Sanderson's Presidency of the British Association.

CHAPTER VIII

LIFE AT OXFORD

1882—1905

The twenty-three years spent by Burdon Sanderson at Oxford, from his appointment in 1882 till his death in 1905, were full of work and interest. The Waynflete Professor soon became identified with Oxford life, and it was not long before he was one of its best-known figures. Though his life was always a busy one, filled up, as might be said, almost to the brim, these latter years were not lived with quite the same strenuous eagerness not to lose a moment of precious time as were those that went before. The atmosphere perhaps was different, and the work undertaken was much more varied: constant journeys up to London were necessitated, and Sanderson was thrown much more into contact with other men in carrying on the business of the University, and serving on Commissions and Committees outside. Then ever after 1884 there were constantly recurring attacks of pain that caused cessation of work for several days at a time, and which absolutely necessitated a certain amount of care. Few people knew how constantly he suffered from pained and sleepless nights, which were specially trying to one of his intensely sensitive nature.

At Oxford, as in London, Burdon Sanderson gathered around him a number of young men as assistants and friends in his work. Francis Gotch (who afterwards succeeded him as Professor of Physiology) and F. A. Dixey accompanied him from London, and taught practical Physiology and Histology in rooms lent by the Regius Professor of Medicine, and afterwards in the new Laboratory. Mr. Gotch also took part with him in electro-physiological

investigations, two of which, on the electrical organs of the Torpedo and the Skate, were carried out respectively in the Marine Biological Stations at Arcachon and St. Andrews during summer vacations. In 1887 Burdon Sanderson's nephew, John Haldane, settled in Oxford, and began to teach Chemical Physiology on the completion of the Laboratory. Others in later years who took part in the teaching were G. A. Buckmaster (now Lecturer at University College, London), Stanley Kent (now Professor at Bristol), M. S. Pembrey (now Lecturer at Guy's Hospital), and Leonard Hill (now Lecturer at the London Hospital). In private research work Burdon Sanderson had the co-operation of G. J. Burch, and latterly of Florence Buchanan, a daughter of his old friend Sir George Buchanan. For the last three years of his life he was also helped by H. S. Souttar, who supervised the construction of a new photographic recording apparatus for the Capillary Electrometer, which could be used without being in a dark room—thus an improvement on the apparatus he had formerly used in London and for the first twenty years in Oxford. Besides his assistants, many other Oxford pupils, or friends from elsewhere, carried out the research work in the Laboratory while he was Professor. The number of students, at first small, increased steadily, and it is remarkable how large a proportion of them have already become prominent in their profession.

From the outset Burdon Sanderson laid great stress on the importance of research work, and himself set an example of the highest standard of painstaking care and accuracy. For everything of the nature of the more or less charlatan science which often passes muster among the general public in this country, he had the utmost contempt, and expressed it without hesitation. His critical regard for accuracy and completeness may sometimes have overshot the mark, but his influence, both on his own pupils and on all who were brought into touch with him in connexion with scientific work, was unmistakably inspiring.

He seemed to impart new value and dignity both to what had already been accomplished by others, and to what remained to be done. Many scientific men have been more fertile in new scientific ideas or in experimental work actually accomplished, but few have done as much to inspire real and lasting respect for the value of scientific research. The line of experimental research to which he devoted his spare time at Oxford was highly specialized, and surrounded with difficulty; but he was no mere specialist, and his influence on pupils and friends was undoubtedly very far-reaching. His personality seemed to stand for the highest ideals in many different directions.

His lectures were always prepared and delivered with great care; but the words did not flow easily, and many of his hearers found it difficult to follow—partly, perhaps, because they were somewhat awed by his manner, and looked for deeply hidden meaning in what he meant to be nothing but plain and simple statements. When he thought it necessary to repeat a statement it was usually in quite different language, and this sometimes added to the difficulty. To those, however, who were really interested his lectures were very inspiring, for he was plainly in living touch with every part of his subject. To practical experimental work in the Laboratory he attached the greatest value, and by far the greater part of the time spent by students in the Physiological Department was devoted to it. The practical courses were necessarily conducted mainly by his assistants, but he regularly took part in one of the main practical classes and frequently visited the others.

In 1889 a much-loved sister died in India, where she had gone to spend the winter. Jane Charlotte Burdon Sanderson was seven years younger than her brother, and the youngest of her family. There was always a strong bond between the two, and this bond was strengthened by the fact that the sister was delicate in health and required all the love and care that could be lavished on her. She was of an intensely sympathetic disposition, and had the

power of attracting men and women of very varied points of view. She had also a wide intellectual outlook and possessed speculative interests, which were manifested in the work she did in translating Hegel's *History of Religion.* The work was left incomplete, but was finished by another hand after her death.

His sister's death was an intense grief to Burdon Sanderson, as his letters show. She and he had carried on a close correspondence for many years, and when she was in England he paid frequent visits to her home in Bournemouth. The entry in his diary on the night on which he heard of her death is eloquent testimony of the love he bore to her; he writes the simple words, 'The saddest evening in my life.'

A large amount of Burdon Sanderson's time was, of course, occupied with his own work in the Laboratory, in lecturing and examining, and in arranging the work that fell to him in connexion with medical studies. But he was always ready to walk or drive with a congenial companion in the country round Oxford, sometimes on early summer evenings to hear the nightingales sing. A concert either in London or Oxford was an immense pleasure both to his wife and himself, and now and then they went to the Play and thoroughly enjoyed it. Besides this they delighted to entertain their friends and be entertained by them. Burdon Sanderson was too much devoted to his work and home to use his Club to any great extent, but when in London he often visited the Athenaeum. He also belonged to certain other societies of a semi-social kind, one of which, 'The Philosophical', he joined in 1877, and its meetings he continued to attend with evident enjoyment.[1]

[1] Note by the late Sir Francis Galton: 'The Philosophical Club was founded in 1847 to commemorate an important reform of the Royal Society, a reform which was narrowly carried after long conflict by the progressive members of the Society. Its principal feature was to limit the number of Fellows elected annually, and to assign the nomination of candidates to the Council. Previously their election had practically been the act of the President alone, and persons were not infrequently

In vacation time he sometimes lectured to the University Extension students, and found his audience interested and appreciative.

In 1886 7 a Committee was appointed to investigate the question of hydrophobia in the light of Pasteur's work, on which Burdon Sanderson served. He had been interested in this subject beforehand, and had written a letter to *The Times* on the steps which should be taken when a bite was received from a suspected dog. In 1886 he visited the Pasteur Institute in Paris for the Committee on Pasteur's Experiments. Sir Victor Horsley writes in regard to this Committee:

'Burdon Sanderson was always keenly interested in the precise determination of the infection in hydrophobia as an instance of a contagious disease comparable with that which attracted his attention for so many years, namely, pleuro-pneumonia. In 1877, chiefly at his suggestion, the British Medical Association, which had recently instituted a Committee to administer grants from the funds of the Association for the promotion of scientific investigations, commenced an elaborate inquiry into hydrophobia or rabies, and appointed a Committee for this purpose. The British Medical Association expended ultimately a sum of £150 on this subject.

'The Committee published in 1878 the first interim report, in 1879 a brief reference in the report of the Scientific Grants Committee, and in 1880 a second interim

admitted who did not deserve the honour; so the value and reputation of the Society had greatly diminished. The reform fully remedied all this. A group of the foremost "Progressives" formed themselves into a dining club, which was limited to the number of 47, to recall the year of the reform. At each meeting after dinner the members were invited to describe briefly recent scientific events, and minutes were made and kept of what they said. It became a considerable honour to be elected into the club. There were at the time of which I am speaking two dining clubs which met on alternate fortnights, viz. The Philosophical and the much older Royal Society Clubs; these gradually grew to be of equal importance, the motives of the original severance were forgotten, and the clubs were to a considerable extent composed of the same individuals. Ultimately, and not very long ago, the Philosophical Club was merged into the Royal Society Club, which now meets weekly on Thursdays on the same days as the Royal Society.'

report. In 1878 and 1880 the anatomical specimens and microscopic preparations accumulated by the Committee were shown at the annual meetings by Sir William Gowers and Dr. Turner.

'Very little later Pasteur began by attempting in 1880 to determine the virus of rabies and, as is well known, he proved by his experiments that the virus was present in large quantity in the central nervous system, that it could be handed on from animal to animal, causing symptoms punctually at the end of an ascertained incubation period. He next established the fact that he could protect dogs by inoculating them from the dried spinal cord of a rabid rabbit. He had previously found that by drying the virus could be weakened and used therefore as a vaccine.

'The work of the Committee of the British Medical Association was not continued beyond 1880, but after M. Pasteur's demonstrations in 1885 the English Government on April 26, 1886, appointed a Commission to investigate the facts. The Commission was under the chairmanship of Sir James Paget, and the two most active members were Dr. Burdon Sanderson and Sir Henry Roscoe.

'The Commission carried out an inquiry into the patients treated by M. Pasteur, and also started through Sir Victor Horsley, who was the Secretary of the Commission, a series of experimental investigations at the Brown Institution. Burdon Sanderson, whose sympathies were always very strong towards the school in which he had received his earliest and best training in Pathology, visited Paris and made a close study of the subject at the Pasteur Institute. The Commission fully confirmed M. Pasteur's results, and in reporting urged upon the Government to adopt the views of M. Pasteur in respect of the particular conditions prevailing in the United Kingdom, and by reason of which it ought to be possible to stamp out the disease entirely.

'M. Pasteur's suggestions amounted to (1) the institution of a quarantine period for imported dogs, and (2) the universal application of the muzzle to stamp out existing infection. The Government accepted these views, in spite of the opposition of the anti-vivisectionists, who held public meetings and endeavoured to revive the old view that rabies or hydrophobia was a form of insanity in the

dog and was not an infective disease which could be dealt with, as Pasteur had proved, by usual medical means. The provisions of the Board of Agriculture proved adequate, and the United Kingdom has for many years been free from this terrible disease.'

Another question which occupied Burdon Sanderson greatly a few years after he came to Oxford was the constitution of the London University. As the result of the movement started in 1884 in favour of the establishment of a Teaching University for London, a Royal Commission was appointed in 1888, of which Lord Selborne was chairman. This Commission had before them certain petitions for a Charter for a Teaching University in London, and in their Report they expressed the opinion that the general case for a Teaching University in London had been proved, and they made certain recommendations as to the scope and constitution of such a University, but they recommended that a reasonable time should be allowed for the Senate and Convocation of the University of London to consider whether they would apply for a new Charter extending the function and duties of the existing University of London, which was at that time merely an Examining University. The University authorities, however, were unable to agree upon any scheme, and the petitions for a Charter came again before the Privy Council. After its opponents had been heard, a Draft Charter was approved and laid upon the table of each House of Parliament.

The University proposed to be founded under this Charter was to be called the Gresham University. An Address was presented by the House of Commons against the grant of the Charter until it should have been remitted for further consideration and report to a Second Royal Commission. This Commission was appointed on the 30th April, 1892, under the chairmanship of Earl Cowper. The other members of the Commission were Lord Reay, Bishop Barry, Sir Lyon Playfair, Sir William Savory, Sir George M. Humphry, Professor George Ramsay, Rev. Canon

Browne, Professor Sidgwick, Professor Burdon Sanderson, Mr. James Anstie, Mr. Palmer, and Principal G. H. Rendall. The reference to the Commission was to consider and, if they should think fit, alter, amend, and extend the proposed Charter for the Gresham University referred to in the Address of the House of Commons to the Crown, so as to form and report a scheme for the establishment under Charter of an efficient Teaching University for London. The Commission presented their Report on the 24th January, 1894, recommending that there should be one University for London, which should be both an Examining and a Teaching University. They also made very full recommendations as to the constitution and work of this University. Burdon Sanderson took an active part in the proceedings of the Commission, and signed the Report of the Commission without any qualifications, although a number of the members of the Commission who signed it did so subject to explanations and qualifications appended to the Report.

The Report of the Gresham Commission met with much opposition from existing graduates who either resisted change or desired to have more control of the new University; from advocates of a Teaching University limited to Colleges of University rank; and finally from representatives of polytechnics and other excluded institutions which they desired to be brought within the scope of the proposed University. Progress seemed hopeless, but negotiations were set on foot which resulted in a compromise, and the Bill became an Act. The Report thus remained the general basis in accordance with which the Statutes and Regulations for the reconstituted University of London were prepared by the Statutory Commissioners under the University of London Act, 1898; but its provisions became subject to many important modifications imposed upon the Commissioners by the Schedule to that Act, and Lord Cowper's Commission cannot be held responsible for certain difficulties which have arisen in the working

of the constitution of the reconstituted University of London.[1]

Although the best years of his life were devoted to the pursuit of Physiology, Burdon Sanderson's sympathies were by no means confined to this subject but extended to all branches of medical science. Indeed, as we have already seen, his earliest scientific work dealt with problems relating to public health, a subject which continued to enlist his warmest interest throughout life. It is only natural, therefore, to find that the project to found an Institute of Preventive Medicine in England was one which appealed strongly to him.

Dr. C. J. Martin, the present Director of the Institute writes:

'This scheme, in the development of which he took an active part, arose from a meeting held at the Mansion House, London, on July 1, 1889, for the purpose of taking steps to present M. Pasteur with a grateful acknowledgement from this country of his gratuitous kindness in Paris to over 200 British patients who had been bitten by rabid animals. The acknowledgement took the form of a donation of £2,000 to M. Pasteur for the use of the *Institut Pasteur* in Paris. At the same time the Committee realized the want in the United Kingdom of an institute similar in character and purpose to the *Institut Pasteur* in Paris, or to the Hygienic Institute in Berlin, and others established on the Continent for scientific research into the causation and prevention of the various infective diseases of men and animals.

'With the idea of meeting this need the British Institute of Preventive Medicine was incorporated on July 25, 1891, and the objects of the Institute were set forth in a Memorandum of Association, viz.:

'*a*. To study, investigate, discover, and improve the means of preventing and curing infective diseases of man and animals; and to provide a place where research may be carried on for the purposes aforesaid.

'*b*. To provide instruction and education in Preventive

[1] Much of the above information was kindly supplied by Mr. Kemp, Secretary to the Royal Commission now sitting on the London University.

Medicine to Medical Officers of Health, medical practitioners, veterinary surgeons, and advanced students.

'*c.* To prepare and to supply to those requiring them such special protective and curative materials as have been already found, or shall in future be found, of value in the prevention and treatment of infective diseases.

'*d.* To treat persons suffering with infective diseases, or threatened with them, in buildings of the Institute or elsewhere.

'*e.* With a view to effecting these objects to provide laboratories, to appoint a scientific staff, to institute lectures and demonstrations, to issue publications of the transactions of the Institute, and to found a library.

'Burdon Sanderson was an original member of the Council of the British Institute of Preventive Medicine, upon which body he represented the University of Oxford. Among his colleagues were Sir Joseph Lister, as Chairman, Sir Henry Roscoe, Treasurer, Huxley, Ray-Lankester, Horsley, Watson Cheyne, and Sir Andrew Clark. The first Director of the Institute was Dr. Armand Rüffer. From the earliest times until shortly before his death he was a regular attendant at the meetings of the Council and took a most active part in the management of the Institute. The office of a member of Council was for many years no sinecure, and the direction of the new Institute was an anxious task. During its early years it had great financial difficulties to contend with, and on more than one occasion its continued vitality was almost despaired of. It was only by means of the enthusiasm and careful guidance of its Council, and the self-sacrifice of the small body of scientific men which composed its staff, that it did not succumb to inanition. It was also unfortunate in changing its birth-name more frequently than is good for a young institution. In 1898 it became the Jenner Institute of Preventive Medicine in order to receive the donation of a sum of money collected to perpetuate the memory of Edward Jenner and his work. Subsequently it was found that a trading firm possessed the prior legal claim to this title, and a further change of designation being necessitated it was decided to associate the Institute in future with the honoured name of Lord Lister, who had been the Chairman of its Council since the foundation. It thus in 1905 became the Lister Institute of Preventive Medicine.

'In the meantime the financial stresses which threatened

the collapse of the Institute had been considerably relieved by substantial donations from the Berridge Trustees, the Grocers' Company, and a number of public-spirited men.

'The Duke of Westminster having granted, on terms which meant a large personal contribution, a fine sight facing the Thames at Chelsea Gardens, the Council proceeded to build the present headquarters of the Institute. These were opened in 1897. The permanent income of the Institute was not, however, adequate to the requirements and capabilities of the enlarged establishment, until towards the end of 1898 it received, for the encouragement of research into the cause and treatment of disease, a most generous endowment of a quarter of a million sterling from Lord Iveagh. This endowment enabled the Governing Body to greatly extend the usefulness of the Institute, and to increase the (up to that time) very inadequate staff.

'The development of serum therapeutics in 1894 attracted general interest to Preventive Medicine. A few years previously Behring had discovered that by accustoming an animal to small but progressively increasing doses of tetanus poison, the serum of such an animal possessed the property of neutralizing considerable quantities of the poison. This discovery was amplified and put to practical use for the treatment of diphtheria by Roux and Ehrlich, and rapidly established itself as the only rational and effective treatment for this disease.

'The preparation of antitoxic sera was at once taken up by the Institute. Some temporary premises near London where horses could be accommodated were acquired, and, as soon as the value of the remedy was established, the Council purchased a freehold property near Elstree, Hertfordshire, where a very complete equipment for research into serum therapeutics was installed.

'As previously mentioned, Burdon Sanderson, as a member of Council, was actively concerned with every development of the Institute. There were, however, two directions in which his influence was particularly marked. The first was the establishment, in the early days, of courses for training medical men, and particularly those intending to qualify themselves for appointments in public-health service, in bacteriology and hygiene; sciences which he clearly saw would ultimately supply the solution of the problems confronting them in their daily work.

'The second important influence which he exerted as

a member of the Governing Body was to broaden the conception of the basis of Preventive Medicine. He held that, although the discovery of the microbial origin of disease was of fundamental importance, the means whereby microbes exerted their deleterious effect, the defence aroused against the invader and the possibility of controlling defence-processes, were matters which would likely come as much within the domain of Chemistry as Biology.

'It was largely owing to his advice that the chemical side of the Institute was strengthened by the foundation of a laboratory for Physiological Chemistry.

'The advantages which the Institute derived from its association with Burdon Sanderson were not by any means confined to his wisdom as a councillor. To the scientific staff he was always a helpful colleague. Whatever the nature of the problem with which he was concerned, the worker was sure of his sympathy, and his great knowledge and critical insight were ever at the disposal of the humblest investigator.'

Burdon Sanderson was also a member of a Royal Commission appointed to inquire into the effect of food derived from tuberculous animals on human health, which reported in 1895. The Commission had been appointed in the first place in 1890, under the chairmanship of Lord Baring, and was reappointed in 1894, after Lord Baring's death, with Sir George Buchanan in his place. The Commission was directed to report as to 'what is the effect, if any, of food derived from tuberculous animals on human health; and, if prejudicial, what are the circumstances and conditions with regard to the tuberculosis in the animal which produce effect on man.' Professor M^c^Fadyean made a special inquiry for the Commission into the means of recognizing tuberculosis in animals during life; Dr. Sidney Martin into the influence on lower animals of food of tuberculous origin; and Dr. Sims Woodhead into the effect of cooking processes upon food from tuberculous animals. In order to make these inquiries experiments were largely made on the lower animals, since it was naturally impossible to do so upon men. The main conclusions arrived at were that food derived from tuberculous animals can produce tuberculosis

in healthy animals, and that the actual amount of tuberculous disease among certain classes of food-animals is so large as to afford to man frequent occasions for contracting tuberculous disease through his food. The circumstances and conditions with regard to the tuberculosis in the food-animal which lead to the production of tuberculosis in man are, ultimately, the presence of active tuberculous matter in the food taken from the animal and consumed by the man in a raw or insufficiently cooked state. It was likewise found that tuberculous matter is seldom discovered in the meat substance of the carcase, but principally in the organs, membranes, and glands. Tuberculous matter in cows' milk (when the udder has become invaded by tuberculous disease) is exceptionally active in its operation on animals fed with the milk, and therefore the drinker of raw milk is exposed to danger, a danger which could be avoided by boiling even for a moment. Ordinary processes of cooking applied to meat which has got contaminated on its surface are probably sufficient to destroy the harmful quality, but they would not avail to render wholesome any piece of meat that contained tuberculous matter in its deeper parts.

This Commission entailed much work upon its members, and Burdon Sanderson attended the meetings very regularly; but the subject was one which interested him deeply, both from the purely scientific point of view and that of the help that should be given to the tuberculous poor. As a result of his interest in the subject a branch of an association dealing with the matter was established at Oxford in 1899. It had as its object the attacking of the two special dangers which Burdon Sanderson believed to be at the basis of the disease—bad sanitation and bad milk. The remedy he suggested was voluntary notification or registration. But he felt that one of the great problems in connexion with the eradication of the disease was the provision of suitable sanatoria for the tuberculous poor. Both he and Sir John Simon wrote forcible letters on this

subject to *The Times* in 1901. Burdon Sanderson argued that the only means of attaining the object was by the establishment of sanatoria for the breadwinners of both sexes. Houses of recovery must be provided in healthy situations not too far removed from industrial centres, where working men and women threatened with phthisis might obtain such accommodation as they require. Admission must be free to those who are socially and medically eligible, and the number of beds must be sufficient to render possible the prompt admission of all who require it. He had already delivered a lecture giving an account of how the battle against consumption had been waged in Germany, by means of such methods as are indicated above, in connexion with national insurance and private charity. If it were possible to introduce similar sanatoria treatment in England he believed that much good might be done.

A matter of the same nature in which, from his previous knowledge, he was well qualified to give valuable assistance, had regard to an inquiry made by the Board of Agriculture through its President, the Right Hon. Herbert Gardner, now Lord Burghclere, into the landing in Great Britain from Canada of cattle affected with pleuro-pneumonia. This inquiry took place in May and June of 1894.

In 1893 Burdon Sanderson presided over the meeting of the British Association held at Nottingham. The presidential address, dealt with elsewhere, was a lengthy one, and to those competent to follow it a very interesting one. But it may be feared that it was somewhat too recondite for the ordinary listener, and the writers of the lay press prudently shirked the task of commenting on it as is their wont. The personality of the man probably impressed his audience more than what was stated to be a 'somewhat indigestible meal of intellectual food, even for those who have dipped into biology and cognate sciences.'

Ceremonies and fêtes of various kinds were not as a rule appreciated by Burdon Sanderson, to whom simplicity of

life was an essential, but he attended the various functions that were indispensable; and the nature of the reception he received could not but have been gratifying to him.

On the resignation of Sir Henry Acland in 1895, the Regius Professorship of Medicine became vacant and Lord Rosebery appointed Burdon Sanderson to succeed him. The appointment was one which was generally approved, as it was felt that Sanderson would be able to consolidate the work of the School which, in its new development, he had done so much to create. It relieved him of the somewhat heavy teaching duties of the Chair of Physiology, but brought with it many new responsibilities; and to fill Acland's place in the University was no easy matter. As already mentioned, the main teaching of the various branches of Medicine and Surgery is not undertaken by Oxford University. But the examinations in these subjects are conducted there, and at Oxford the whole course of instruction of future graduates in Medicine must be arranged with reference to the requirements of their future work. The responsibility for these arrangements, for generally superintending the examinations and the acceptance or rejection of theses submitted for the degree of Doctor of Medicine, falls largely on the Regius Professor, who also acts as Chairman of the Board of Faculty of Medicine. Burdon Sanderson took these duties very seriously, and perhaps at no period of his life was his wife's constant help of more immediate value to him. But for her, his absent-mindedness would often have landed him in formidable difficulties.

As Regius Professor he had an official position in connexion with the Radcliffe Infirmary, having the right to sit on the Board of Management. He interested himself much in its affairs, and when able attended the weekly and monthly meetings. He also had duties in reference to the Ewelme Hospital, of which he was *ex-officio* Master, and he frequently visited the picturesque Almshouses, both as a matter of duty and as a pleasure.

On entering upon his new work he found himself called upon to develop the teaching of Pathology, a matter which could not have been taken up as a subject of teaching in Oxford unless Burdon Sanderson had persistently kept the matter before the University during the first year of his Regius Professorship. The subject was specially congenial and interesting to him, as has been seen. He was President of the Pathological Society of London from 1902 to 1906, and put himself to great pains to preside regularly at the meetings, and later took part in the discussions.

Dr. James Ritchie, now Superintendent of the Research Laboratory of the Royal College of Physicians of Edinburgh, who developed the teaching work in Oxford under Sanderson's direction, and was afterwards appointed to the newly instituted Chair of Pathology, writes in reference to his work in Pathology there as follows:

'Up to the time when Burdon Sanderson was appointed Regius Professor of Medicine, the subject of Pathology had not been taught in the Oxford Medical School. Even in the examinations for the Bachelor of Medicine degree, Pathology had only begun to be differentiated as a special branch of medical training. For some years previously one special examiner had acted in the degree examinations, and about 1895 the subject was put on a footing equal to that of the other subjects in the Final Examination by having two examiners allotted to it. A beginning had been made in 1895 with the teaching of the subject when, under the auspices of Sir Henry Acland, a Practical Class in Bacteriology had been conducted. But one of Burdon Sanderson's first acts after his appointment in 1895 was to arrange for a complete course of instruction in Pathology and Bacteriology, extending over the three terms of the academic year; in this course he himself took an active share by delivering during one Term twelve lectures on the theoretical aspects of the subject. Otherwise the teaching was chiefly practical.

'The difficulty at the outset was the provision of accommodation, and to meet this the Regius Professor gave up to the teaching the four small rooms in the north-west corner of the Museum Court officially allotted to him. In order to provide material for the Practical Class, Sir John took

advantage of his being *ex-officio* Physician to the Radcliffe Infirmary to voluntarily undertake the responsibility for post-mortem examinations in that institution, the work here being handed over to a deputy. Very soon the accommodation for teaching became insufficient, as the number of students necessitated the teaching of a Practical Class in three rooms at one time, which was, of course, an impracticable arrangement. The question of the provision of a proper Department of Pathology thus arose in an acute form, and during the years from 1896 to 1899 he was constantly agitating in the University for such a Department being instituted.

'In 1896 a Committee of the Museum Delegates, presided over by the President of Magdalen, had been considering applications from the Linacre Professor of Comparative Anatomy and the Sherardian Professor of Botany for increased accommodation for the teaching of Botany and Zoology, and Burdon Sanderson took the opportunity of moving the Delegates to extend the remit to this Committee, so that the subjects of Pathology and Pharmacology should also be considered by them. This the Delegates agreed to do, and a plan was submitted to them showing how part of the new building proposed for Botany and Zoology could be utilized for Pathology and Pharmacology. Objections were, however, raised to this scheme of a conjoint building, and the matter fell into abeyance. But it was raised again in November of the same year by Burdon Sanderson addressing a letter to the President of Magdalen, in which he stated the position of the University of Oxford towards medical science, and insisted upon the necessity of provision being made for the teaching of all the strictly scientific branches of Medicine.

'The Committee reported in favour of making suitable provision for the teaching of Pathology, and in the same month their Report was approved by the Museum Delegates, who, in transmitting it to the Hebdomadal Council, submitted it as desirable that a Department of Pathology should be constituted with as little delay as possible. As no action was immediately taken, the Regius Professor again took the matter up in October, 1897, and addressed the Vice-Chancellor, now urging that the space at that time occupied by the Radcliffe Library, for which a new building had been acquired, should be given over to Pathology and Pharmacology, and at this time a plan

was submitted to the Museum Delegates showing how the old library could be adapted for such new purposes. The matter was again delayed, and in Michaelmas Term, 1898, Sir John again moved the Delegates to take the question up. This time he submitted two alternative plans, one under which the old Radcliffe Library would be utilized, and another in which a larger scheme was promulgated for the erection of a separate and complete Pathological Institute outside the Museum to the south of the Department of Human Anatomy. These schemes were considered by a Sub-Committee, consisting of the President of Magdalen and the Junior Proctor, Mr. Bourne. Admitting the feasibility of the plan for adapting the library, they recommended that the larger scheme should be entertained.

'Plans were drawn up for the carrying out of this larger scheme, but of course the difficulty of the situation lay in the expense which necessarily would have to be incurred. Still we find the Regius Professor keeping the attention of the University fixed on the matter, and in June, 1899, he issued a Memorandum, pointing out the necessity for provision being made for teaching and research in Pathology. In this Memorandum the fact was insisted on that the University of Oxford was the only great University in Britain in which such provision was not made. In the same year the financial difficulty was solved by Dr. Ewan Fraser of Balliol (at that time a young graduate in Medicine, who had been a member of the first classes in Pathology which had been conducted in the University) coming forward, at first anonymously, with an offer of a sum of £5,000 for the building of a Pathological Laboratory, on condition that the University provided a similar amount. The gift was accepted, and thus the sum of £10,000 was available for the equipment of a Department.

''The building was at once begun, and was opened on October 12, 1901. On that occasion, Burdon Sanderson stated that he rejoiced most heartily that they had that day been able to place the keystone of the arch of their scientific studies in the University. During the whole time in which the building was in course of construction he took the keenest interest in every detail, and much of the completeness of the internal arrangement was due to his suggestions and criticisms.

'During these years the development of the teaching of

Pathology had gone steadily on. On Burdon Sanderson's instigation, in 1897 the Delegates of the Common University Fund instituted a Lectureship in Pathology, and in 1901 this was developed into a University Readership. During these years also, and even after the new building was opened, he continued to take an active share in the work by delivering lectures, and in fact these constituted his Statutory Lectures as Regius Professor of Medicine.

'During his later years, the building in the erection of which he had taken such a part was the place of his latest scientific labours. He had a room in it assigned to him, and here he built up an improved photographic recording apparatus, in the elaboration of which nearly three years' constant laborious work was expended. It was the great regret of his last days that while the apparatus was then almost completed, he would not have the opportunity for applying it to the objects for which it was constructed.

'There is another matter in connexion with his work as Regius Professor to which allusion may be made, namely, the active steps he took for bringing the members of the medical profession in Oxford, and especially the staff of the Radcliffe Infirmary, into close touch with the Medical School. In his speech at the opening of the Pathological Department he said: "What they had to do was to bring scientific studies into relation with practice, to unite science with practice, to bring the medical staff of the Hospital into relation with the Professors at the Museum and the work done in the Hospital with the work they were doing in Pathology in that laboratory." He carried out such an aim in a variety of ways. He took the greatest interest in the Oxford Medical Society by coming to its meetings and acting for some time as its President. He was a frequent attender at the weekly meetings of the Infirmary Board, and every scheme for the increase of the efficiency of the Hospital found in him a warm supporter. He thus did much to maintain a high standard of professional attainment in the city and its surrounding districts.'

Professor Gotch, in the Obituary Notice for the Royal Society[1] quoted before, gives the following account of Burdon Sanderson's later scientific work at Oxford:

[1] From the *Proceedings of the Royal Society*, 1907.

'The most extensive of the researches which he carried out at Oxford were upon muscle. In order to obtain exact records of the electromotive changes during muscular activity he devoted great attention to the working out of an adequate method. Inspired by his enthusiasm, Burch took in hand the capillary electrometer, greatly improving its construction and developing a method by means of which the graphic records of its well-known displacement could be interpreted with accuracy. It thus became possible to construct, from the actual photographic records, other derived curves which should faithfully display the variations in the potential difference of a tissue even when these variations were of very brief duration.

'The whole method was elaborated by Burdon Sanderson with scrupulous attention to accuracy of detail; he left it the most exact method of its kind employed in this or any other country, and the utility of the technique has now received ample acknowledgement from all those who are engaged in investigations upon the electromotive phenomena displayed by excitable tissues. Others, inspired by his example and encouraged by his help, have applied the method with success for the elucidation of the electromotive changes occurring in nervous and other tissues, including those which are found in the electrical organs of fishes. As regards the last-named structures, Burdon Sanderson himself visited Arcachon in 1886, St. Andrews in 1887 and Plymouth in 1888, in order to investigate the electrical organs of fishes, and published, in conjunction with F. Gotch, two papers upon the electrical organ of the Skate in the *Journal of Physiology*.

'The results of the investigations upon muscle are given in the papers which he published in the *Journal of Physiology*, and a comprehensive account of the work and of his conceptions as to the excitatory process in this structure, is set forth in the third of his Croonian Lectures.

'This lecture, delivered in 1899, was entitled "The Relation of Motion in Animals and Plants to the Electrical Phenomena associated with it." It deals first with the technique by means of which he endeavoured to obtain precise information, and then sets forth the main results obtained by its employment in *Dionaea* and in striped muscle. The electrical response is described in various forms of muscular contraction such as twitch, tetanus and reflex activity, also in the peculiar condition produced by

veratria. In summarizing the indications which the phenomena seemed to him to afford he stated that "in striated muscle the primary effect of every excitation is a process of oxidation having its seat at the excited part. It may be surmised that this consists of two stages, namely, liberation of previously intramolecular oxygen, and actual oxidation." He further concludes that "the monophasic variation may be taken as the type from which all other forms of response to stimulation may be derived, either by repetition, prolongation, or interference."

'An interesting point for electro-physiologists is associated with the concluding sentence, which affirms that he believes the "muscle current" to be "the manifestation of processes which have their seat at the surface of contact between electrode and living muscle"; he thus definitely adopted the view which is termed "pre-existent" as opposed to the "alteration" conceptions of Hermann.

Miss Florence Buchanan, who assisted in the electro-physiological work for the last nine years of Burdon Sanderson's life, writes:

'Five years before the publication of his 1895 paper, entitled "The Electrical Response to Stimulation of Muscle and its Relation to the Mechanical Response", he had published, both in the *Physiologisches Centralblatt* and in the *Proceedings of the Royal Society*, short papers on the same subject, in which, by an oversight in reasoning which he himself could never understand, he drew the conclusion that the electrical response does not precede the other in time, as had until then been believed, and as he himself afterwards always believed and stated. Unfortunately, before the appearance of his paper in the *Journal of Physiology*, xviii (1895), in which (on p. 148) it is shown, correctly, that there is really *no* measurable latency in the electrical response as there is in the case of the mechanical response, the statement he had first made had found its way into one textbook after another, and even into Prof. Biedermann's well-known work on Electro-physiology. This illustrates the prestige that went with his name, for the point was an important one, and had any one really studied the description he gives of the experiments on which the conclusion drawn in 1890 was based, instead of merely accepting the conclusion, he would have seen that the responses compared

were obtained under such different conditions that no relation between the times of their occurrence could be established from them. So little did Burdon Sanderson himself remember the conclusion he had drawn before, that while tacitly correcting it, he never referred to it in his later paper, and it was not until 1902 that the discrepancy between the two statements was brought to his notice. Dr. (now Professor) Durig, of Vienna, asked leave in that year to come and work under his direction, and drew Burdon Sanderson's attention to the forgotten paper. So obvious did the fallacy now appear that it was difficult for Burdon Sanderson to believe that he had ever overlooked it, but he at once wrote a passage for Durig to insert as a "briefliche Mittheilung" in his own paper on the work he had done in Oxford, expressing regret at the error which had occurred. It is difficult for any true man of science in the thick of a quickly developing new line of inquiry to be always consistent; for so absent-minded a man as Burdon Sanderson it was practically impossible.

'Two short papers, written jointly with Miss Buchanan, on the reflex spasm of strychnine, were the last electro-physiological ones that he published; these appeared in 1902, one in the *Physiological Society Proceedings* and the other in the *Physiologisches Centralblatt.*

'During the last years of his life Burdon Sanderson was himself engaged, whenever his health and other work permitted, in writing a paper which was to embody the results of all his electro-physiological work, and to bring out the significance underlying the phenomena. The experimental work was confined to the repetition of all that he has called in his published papers "fundamental experiments", so anxious was he that the basis of the theory he was going to set forth should be absolutely sound. The manuscript of this paper was unfinished and fragmentary at the time of his death, and, valuable as such a summing-up of his work and of his views as he proposed would have been from his own pen, it would have lost its *raison d'être* in the hands of another.

'He was also very anxious that his old friend *Dionaea* should vindicate itself, and he procured a consignment of the plant from America in the summer of 1903. He had hoped that by that time the apparatus which he had used for so many years for recording electromotive changes in animal tissues, but which he had relinquished in July, 1902,

would have been replaced by other apparatus which promised to be much more manageable. It was a great disappointment to him that the *Dionaea* had to wait several months for the lack of any usable recording apparatus at all. With the care he bestowed upon them, however, the plants were still in fairly good condition in November, 1904, when the recording of the electrical response to excitation became again possible. It was then with great delight that he saw once more and displayed to his friends, photographs, taken with quicker and more sensitive electrometers than had been available a quarter of a century before, of what at that time had so fascinated him. They did more than confirm his fundamental experiments, but the new facts which they seemed to bring to light are in need of confirmation with plants in a more vigorous state, and have not therefore been published.'

Burdon Sanderson held the Office of Regius Professor of Medicine until 1904, a year before his death, when he felt compelled to resign it owing to failing health. It gave him great pleasure to know that his successor, Professor Osler, was not only an old pupil and friend, but one whose work he had watched from its commencement with satisfaction and admiration.

One of his frequent correspondents through life was his eldest sister, who survived him for three years. Miss Burdon Sanderson was not only in herself a striking personality and the centre of many friends in Hampstead, where she resided, but she was also interested in numerous philanthropic schemes, and more especially in one for the Employment of Epileptics on the Colony System, such as has long been carried on at Bielefeld in Germany. This Society she helped to set on foot after visiting Bielefeld, and her brother took a deep interest in her work and aided her with his advice. Shortly before he passed away he wrote to her as follows:

'It is actually three weeks since we parted. . . . It is a great pleasure to me to write to you, but as I get older "how not to do it" becomes more and more easy as compared with how to do it. . . . I got cold sitting in the Sheldonian Theatre waiting for the Vice-Chancellor, and must take care of myself

for a few more days. Did you notice R. B. H.'s successful meeting yesterday [Friday], and the very excellent address in which he urged the claims of historical study and research. The performance was the more remarkable that he was about to address a political meeting the same afternoon or evening. I grumble at having to keep my room, but have really little to complain of.'

A few days later, on October 13th, he says:

'I have now inhabited this bed for eight weeks and for this time have not [till] to-day put pen to paper. Some days ago I resolved that my first effort in the way of writing should be to write to you. To-day I feel that I can venture. I certainly feel better, although there is very little substantial progress. The only plan I can follow is to limit my looking forward to a day or a night, refusing to ask myself what may happen after. I get through my days without difficulty, for I usually have one or two kind visitors, and we have a piano in the adjoining room on which G.'s cousin, Miss Herschell, plays to me almost every day.

'I often think of you, hoping that you are getting on well, and not forgetting that November[1] is so near.

'Your very loving brother,

'J. Burdon Sanderson.'

Lady Burdon Sanderson wrote to her sister-in-law on November 5th:

'John has not had a very brilliant week. I hope he is once more beginning to go in the right direction. You know perhaps that Richard[2] came to see him for a few hours. It was a great pleasure to John. After three days with no visitors there was an influx on Friday, of whom he saw three. One of them was Mr. Benecke, who played to him for nearly half an hour, which was a great joy. Prof. Osler has just been here. He always takes a cheerful view. Indeed, one does not see why John does not get stronger, as he eats well and sleeps very fairly, and can read to himself with pleasure, besides being read to.'

There was a long time of weakness—physical weakness which had been gradually increasing. But his mind was

[1] His sister's birthday, which was always remembered.

[2] His nephew, now Viscount Haldane of Cloan.

absolutely undimmed, and his devoted wife was able to be constantly beside her loved one and to watch with him to the end.

She wrote on November 24th, the day after he passed away:

'I write just a few lines to say how peaceful the end was. Both yesterday and the day before were very tolerable days for our dear one, but the result was obtained by morphia and his mind was somewhat wandering and confused. Yesterday no morphia was given in the morning, and by the afternoon he was quite himself. He was too weak and exhausted to talk, but he knew me almost to the last.'

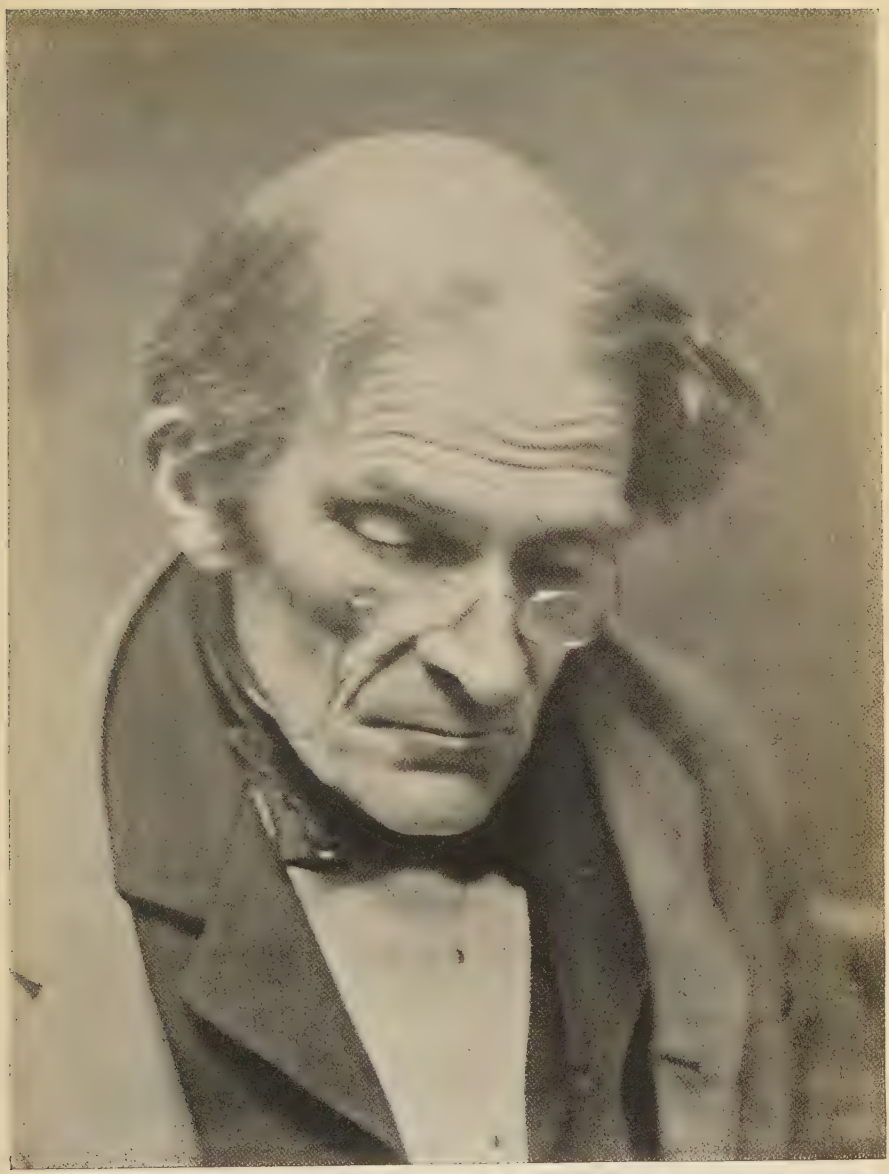
Burdon Sanderson was buried at Oxford, where he died. The funeral was largely attended by members of the University and representatives of learned and scientific societies, as well as private friends. There was a very beautiful service in the chapel of Magdalen, of which College he was an Honorary Fellow, and then the company made its way to the cemetery which he so often passed in his afternoon drives.

The following letter to his elder sister from a nephew expresses what so many felt:

'I know how acutely you will be feeling this severance. It has come gently, but it is the termination of close ties to you and my mother, and the closing of a great life nobly lived. . . . It is not in length of days though that the best lies. It is what has been lived in them while they lasted; and he lived his life up to his ideals and to the limit of his strength—the utmost limit. That is fine and one reverences it.'

The secret of Burdon Sanderson's influence over the younger men with whom he came in contact, and who mourned his loss so deeply, is well expressed in the obituary notice of the *Westminster Gazette*:

'On one occasion a colleague of the Regius Professor was asked to account for his "magnetism". "I attribute his influence over his pupils", he answered, "to his dis-



From a photograph by Miss Acland

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regard for the restrictions of conventions, to the keenness he never fails to display in their work. A man has only to show his appetite for work to be sure of Sanderson's interest and regard. And that in itself is stimulating." The same question was put to a man who had worked under him and was proud to boast himself a pupil of Sanderson. "A good general", he answered, "makes keen soldiers. What one always feels with Sanderson is that he is always leading one on. He takes such a keen interest in one. And one feels that he is infinitely above it all, yet he never fails to show a keenness in one's work, without a trace of condescension or clannishness; that makes one feel one must do one's best if only not to forfeit his regard."'

In his Will Burdon Sanderson desired that the sum of £2,000 be placed in the hands of trustees 'for the support of the Laboratory of the Pathological Department of the University of Oxford, and specially to provide for the expenses of research in Pathology conducted in the said Laboratory or elsewhere.'

CHAPTER IX

PERSONAL CHARACTERISTICS AND OPINIONS

Burdon Sanderson, as has been said, was not a letter writer. Most of his letters were on work being done or other business matters. But those written to near relations have all the attractions of his own personality. Those to his wife are few because he was rarely parted from her, and any that exist are too intimate for publication. One or two to his sisters may be quoted. The first is to Miss Burdon Sanderson, thanking her for a present on his sixtieth birthday:

'Oxford, Dec. 21, 1888.

'I feel that I have turned over a new leaf to-day, and wish to think how best to do it. Perhaps the best resolve that I can make is not to allow the small vexations and disappointments of life to worry me any more! The best way of doing this for me is to see as clearly as I can what sort of work, if any, and how much of it I may hope to do while I have still health and vigour. I do not think I shall ever tire of work, or wish to leave off occupying myself with the pursuits which at present interest me, but I am alive to the conviction that it would not be wise to remain here longer than I can continue to do my work as Professor efficiently. So long as I am here I can do a certain amount of work myself, but my principal satisfaction is to be able to be of service to younger men, and to have the immense advantage of being associated with them in their work.'

The letters that succeed are to his married sister, Mrs. Haldane. The first is written in 1875 on the death of her son, a very promising boy of sixteen:

'There is but one thing that we can do for each other on such occasions as now. We can only feel and express, as best we may, our love and sympathy. It would only be to

do violence to our truthfulness to ourselves to try to persuade ourselves that the cutting off of so precious a life could be anything but a calamity—a grief to us all and to every one who knew him.

'We shall always, to the end of our lives, think what he would have been to us if he had lived, and we cannot wish it otherwise.

'Would that it were possible to bring him back again! but it is not possible.

'May God help you to bear it, and give you so much joy in those that still remain and surround you that in time you may be able to think of the dear life that has passed away without pain.

'To this end, it may be some help to think that here, in Italy, and in many other places, those who love you are grieving with you.

'Your very affectionate brother,
'J. B. Sanderson.'

The next is from a letter of thanks for a birthday present.

'Dec. 21, 1903.

'The beautiful plaid has just arrived. I have already placed it round my shoulders and find it to be most comfortable. It will be delightful for my journeys to London and for the carriage. This is not the season for enjoyment of the fresh air, but it is not the less good for one, and we are trying to use our privilege as often as we can. I am very glad to hear from yourself that you are regaining health, and that you can look back and forward with content. I am trying to do the same, but I find it very difficult, notwithstanding that I have so much to help me.

'We are sending you a Picture Book, which I hope you will like. It contains reproductions of Mrs. Allingham's water-colour drawings by the new three-colour process. I am not so sure as to the success of the drawings of wood and garden,but I think the figure pictures are excellent. That Mrs. A. is *par excellence* a figure painter has perhaps something to do with it, but I think the reproduction of trees is more difficult.

'Your very loving brother,
'J. Burdon Sanderson.'

The following are extracts from letters to the same

correspondent after Burdon Sanderson's health began to fail:

'Oxford, Feb. 18, 1904.

'The few days of sunshine we have had lately have made me wish you were here. The day before yesterday we had a beautiful drive round Boar's Hill. Winter in sunshine *can* be as pleasant as summer. This year the hedges and tree trunks are unusually green—even brilliant—and the flooded Thames valley rather adds to the effect. . . . I think you know that we are under orders for Algiers. As myself a doctor I am under the disadvantage that I am not so prone as the "layman" is to obey without question the advice given me.'

'Algiers, April 20, 1904.

'Just now the weather here is most disagreeable. It is so dark and rainy that, however much we desire to think it pleasant, we cannot. This makes me envy you your faculty of making the best of everything. After all we have not much to complain of, for it is pretty certain that the bright sunshine will soon return. We cannot make up our minds as to the expediency of going to Hamman R'Ihra. When it is fine we have many sources of enjoyment where we are. It has been very helpful to us that we have met with kindness on the part of a variety of people here—both residents and visitors.'

'Oxford, Dec. 8, 1904.

'We have both been rather in a sad way since we parted, but I am very glad to hear that you are mending, though still in bed by the doctor's orders. For the last fortnight I have spent a great part of each day in bed—also by the doctor's orders, and find it very agreeable, for I can do writing work more easily in bed than if I were up. . . . I am going out for the first time in the evening, to the meeting at the Town Hall to hear Richard's speech. I was asked—indeed pressed—by the promoters of the meeting to take the chair, but I felt that my state of health was too uncertain and declined. . . . I shall be on the platform, so that I shall be seen if not heard.

'P.S. Just returned. The speech was admirable. R. made some splendid points and the audience enthusiastic.'

Burdon Sanderson received many honours during his life. He was thrice Croonian Lecturer for the Royal

Society in 1867, 1877, and 1899, and once for the Royal College of Physicians in 1891. In 1878 he was Harveian Orator for the College of Physicians, and in 1880 received the Baly Medal. For special research work and for his general services to Physiology and Pathology he was awarded a Royal medal by the Royal Society in 1883. In 1889 he was President of the Biological Section of the British Association, and he was President of the Association in 1893. He was made a member of the chief foreign Scientific Academies, and in this country held the degrees of D.Sc. of Trinity Coll., Dublin ; D.C.L., Durham ; LL.D., Edinburgh ; in 1899 he was created a Baronet. The degree from the Edinburgh University was given on the occasion of its Tercentenary Celebration in 1892, and Burdon Sanderson's striking figure attracted attention even in that assemblage of eminent men. On receiving the baronetcy Sanderson had, of course, to attend a Levée, and his appearance in Court dress and ruffles was much commented on. The following letter was written by the President of Magdalen to Lady Burdon Sanderson after the function :

'Magdalen College, Oxford,
'May 29, 1900.

'My dear Lady Burdon Sanderson,

'You will have heard and seen by the papers that our ceremony passed off yesterday quite happily. But I must send you a line to tell you how proud I was of Sir John's presence and appearance. To my mind he was far the most distinguished and interestingly distinguished looking figure there. He seemed to me quite beautifully "turned out" with his lace fall and ruffles. He really looked as he ought to and only he can look. He would have done credit to the Court of Elizabeth, but perhaps it is better to do credit to the Court of Queen Victoria. . . .

'Yours very sincerely,
'Herbert Warren.'

Burdon Sanderson had a habit of inscribing mottoes on the notebooks or commonplace books which he kept so

carefully. Some of them are worth quoting, as showing the attitude of mind which they expressed. These are some examples :

'March 25th, 1902.

'So little done, so much to do.'
'Right thou feelest, rush to do.'
'In short measures life may perfect be';

and one of which he was very fond, for reasons which all who knew him will appreciate : 'Le secret de la vie heureuse c'est de travailler . . . et de veiller sans regret.'

He had a great love of children, and these on their part worshipped him when once they overcame a certain awe produced by his height and figure. When dressed as 'Old Father Christmas' he has been known to produce screams of terror from Scottish village children, though the fear was soon happily overcome. One who knew him well as a child says :

'I remember well as girls how we delighted in his company. We were all "held" by his magnetic personality. The mouth would tighten almost to grimness, the eyes narrowing—then a lightning flash would light up the whole face, and you felt the strangely attractive power of his smile . . . We were proud to have him as a playmate.'

His appreciation of his young friends is shown in the following letter to a very young relative :

'Oxford, Oct. 8.

'Dear Rhoda,

'When the cat's away the mice are at play. Your play must I am sure have been very amusing. I wish that I could have had a place somewhere without being myself visible or audible, and whence I could have heard and seen everything.

'As we get old, we have to make believe that we are young, and it gives us great pleasure to be helped in our make-believe by those who can still look forward as you can to happier and happier days, and who are willing to tell us, as you have done in your sweet letter, all about your fun and frolic.

'When I was your age I wished to be older. I do not think that you have any such wish, and I hope that before you become quite a "grown up" you may perhaps come and see your old friends at Oxford.

'Yours affectionately,

'J. Burdon Sanderson.'

Burdon Sanderson's absent-mindedness was a frequent subject of joke amongst the undergraduates, and many amusing tales are recorded, the most amusing probably untrue. There was yet a sufficient foundation of fact on which the tales were founded to form a basis for the reputation he acquired. The reminiscences here recorded may be taken as well-established:

'"The Burder", as he was affectionately called, was always a popular and striking figure in Oxford. Many of the stories of his absent-mindedness in circulation to-day are, of course, apocryphal. But it is an absolute fact that he used to delight his classes by stuffing one chalky duster after the other into his pockets, and finally using his pocket-handkerchief to clean the blackboard. Credible witnesses have seen him at his own dinner-table carefully carve the joint, and serve his guests, and then fall to on his own plate utterly oblivious to the fact that he had forgotten to help himself. The late Sir Henry Acland had a story he always enjoyed of the Professor's absent-mindedness. While a guest at his house, Sanderson one day asked Miss Acland if he might have his luncheon earlier than usual, as he wished to have a walk before returning to the Schools. At the appointed hour lunch was duly served, and the Professor left the house. Ten minutes later, at his usual luncheon hour, he returned, rang the bell, and asked the astonished servant to bring up lunch. It was duly served, and Miss Acland had the satisfaction of seeing her guest make a second hearty repast, utterly ignorant that it was an encore. His wife left him one evening to conduct his guests into the dining-room. When she came down into the hall she found him helping them into their overcoats, shaking hands, and saying good-night. More recently he had a distinguished stranger staying as his guest in his house. Leaving the Museum, he happened to meet his guest strolling towards the Parks. He greeted him with the greatest cordiality,

and invited him to accompany him towards his house. Arrived at the gate, Sir John turned round and shook hands. "It has been the greatest pleasure to me to meet you, and I wish I could ask you to spend a few days with us. But the house is full, as we have some people staying with us."

Other stories, well verified, are of the servitor being called up and reprimanded for having "damp matches again", the fact being that a box of chalks was made use of instead of matches, to the delight of the youthful onlookers. It is also recollected how at University College, when the Professor wished to go upstairs to the room above, he carefully ascended a ladder being used by a painter engaged in painting the roof, greatly to the astonishment of the workman.

In the eighties the feminist movement was coming to the front in England. Burdon Sanderson's view of the relative intellectual powers of men and women is stated in an interesting way in reply to his friend Mrs. Barnett, who asked him to enlarge upon a conversation they had carried on regarding this matter.

Mrs. Barnett writes in December, 1887 :

'We were talking of Romanes' article in the *Nineteenth Century*, and I was telling you how much harm I thought it had done among the class of people who were only too anxious for excuses not to educate their daughters. Prof. Romanes' quotation of Sir James Crichton Brown's opinion that women's brains were lighter by five ounces than men's, and the whole tone of the article, seemed to go to show that women were mentally inferior to men. You said, if I remember rightly, that your own opinion was that the differences generally observed were more due to the different educations given to men and women; and you furthermore spoke of the weight of the brain being in relation to the weight and size of the body. . . . I enclose you a letter from the *British Medical Journal* of September 17, 1887, which in part states what I understood you to say; but if you would just state your opinion of the relation of relative bulk of cerebral mass of women to their intellectual capacity and their muscular development I should be grateful to you."

Burdon Sanderson replied :

'As the muscular system is in closer functional relation with the brain than any other system of organs (the chief function of the brain being to initiate and direct muscular action) I should expect that, if the comparison could be made of the relative weight of brain and muscle in the two sexes, it would yield interesting results. The reason the lives of women are not so *effective* as those of men is therefore not to be sought for in inferiority either of understanding or of imagination. It is to be sought for in other physiological (including of course psychological) differences. The most direct way of getting at it is to consider first what are the qualities on which effectiveness either in man or woman depends. I think that it depends chiefly on the power which the capable man or woman possesses of directing mental effort to a recognized purpose. The essential condition of effective mental effort seems to me to be a *state of contest* between intellectual activity (including imagination, originality, &c.) and that other mental attribute by which we direct that activity to those ends which (if we are possessed of character) we regard it as our life-work to fulfil.

'In this contest the greatest enemies we have to contend with are certain states which are recognized as emotional or sentimental, e.g. depression and elation, sympathy and antipathy, &c., of which the physiological characteristics are that, although they have their seat in the higher machinery of our nervous systems, they affect the lower, and thereby associate themselves with disturbances of our bodily functions, particularly of the circulation. There can, I think, be no doubt that both in men and women the emotions and sentiments, although they furnish us with some of our highest motives to mental activity, are much more often the sources of fatigue, exhaustion, incapacity, and failure.

'It is because women by virtue of their organization are more liable than men to be handicapped by emotion that they will always fail in the race.

'The moral is that if you wish young women to be trained to live effective lives, the first requirement is to confer on them bodily vigour, and the second (and, of course, the most important) to habituate them to the notion of a life directed to the fulfilment of a recognized purpose to which all others must yield.

'And here, as it seems to me, the difficulty arises. For a woman's life is *not* entirely at her own disposal as a man's is. Society requires of men only their muscular or mental work, but of women it requires something more and higher than these. The difficulty seems to me to be that a woman must so hold her life-purpose as to be able and willing to surrender it that she may fulfil a higher duty.'

Pictures were a source of great enjoyment to Burdon Sanderson. There is a rather interesting account of how he came to assist in the naming of Stacey Mark's well-known picture 'Science is Measurement', given by Dr. Buzzard in the following letter to Lady Burdon Sanderson:

'74 Grosvenor Street, W.
'July 7, 1908.

'Dear Lady Sanderson,

'It is long since I wrote about the matter to which you refer, and my impression may easily be faulty, but I think that I asked your husband whether the idea for which I was indebted to him, and which I conveyed by the words "Science is Measurement", originated with him, or whether the notion or definition was one which had been recognized and generally accepted by scientific men. The actual aphorism was my own, founded upon a talk which I had had with him in one of our walks on Hampstead Heath. I had been greatly struck with his remark that Science, in its essence, was largely a matter of weighing, measuring, and otherwise accurately determining. The point of view appeared to me a novel one. When Stacey Marks, not very long afterwards, showed me his picture of a savant standing near a stuffed dodo, or some such bird, I noted the measuring tape and calipers which he had introduced into the scene. I asked Marks what he was going to call the picture (which was going in a few days to the Royal Academy), and he said he had not determined upon a title. He had thought of "The Savant", or "The Philosopher', and asked me whether I could suggest a better one. Mindful of the walk on Hampstead Heath I said, "Call it 'Science is Measurement'." He was much struck with the suggestion, asked me whether that was true, and adopted it.

'My impression is that your husband, in his reply to my

inquiry, said that the title was all very well, as far as it went, but that of course it was only part of the truth. Science was a good deal more than measurement. So I am afraid my aphorism was not scientifically accurate, though it appeared to fit the subject of the picture singularly well; so well, indeed, that I believe the vast majority of those who have seen the picture, either in the original or engraved, have supposed that Marks painted it as an illustration to a well-known aphorism (instead of the converse). Indeed, one scientific man wrote to the painter asking to be referred to the work which contained the aphorism. Marks sent on the letter to me, and I wrote to his correspondent explaining the origin of the title.

'The story struck me as an interesting one, and that is why I have ventured to trouble you with this rather long-winded account. . . .

'Yours most truly,
'THOMAS BUZZARD.'

In earlier days Burdon Sanderson seems to have regarded religious questions from the standpoint of the ordinary orthodox Christian—the standpoint in which he was brought up; later on the problems presented seemed more difficult and the solution not so clear. Naturally reticent, he seldom spoke of such matters, but the following letter to Father Wagget shows the mystical basis on which he considered science and religion could be brought into relation:

'DEAR SIR, 'Oxford, April 30, 1901.

'I feel that I *must* express to you my thanks for the discourse which I had the pleasure of listening to yesterday afternoon.

'I do not mean to say that I was able to follow all that you said as to the identity of method in the two fields of science and religion, but I recognize that the 'mysticism' of which you spoke gives us the only way by which the two fields can be brought into relation.

'Among much that was memorable, nothing interested me more than what you said of Mozley. No one, I am sure, knew better than you the value of his teaching and in what that value consisted.

'Yours faithfully,
'J. BURDON SANDERSON.'

At a much earlier period in his life—about the year 1872—Burdon Sanderson presided over a meeting of the Students' Christian Association at University College, London. Certain notes were left on a half-sheet of paper that evidently refer to his address on this occasion:

'The essence of religion', they run, 'is to have a motive, or, as the Gospel expresses it, to have a "single eye". To be actuated not in matters of so-called religious duty (which I may shortly designate Church matters or Sunday matters), but in all the affairs and duties of life, by the determination to eschew the evil and choose the good—to follow after righteousness. The question is often discussed whether there is any difference between morality and religion, between right doing and right feeling. The great difference evidently lies in this, that morality *may* be based upon self-seeking and self-interest, but religion cannot. We may be moral because we see clearly that it is for our interest, directly and indirectly, to be so: directly, because honesty is the best policy; indirectly, because for the community at large regularity of behaviour is advantageous—but religion is different. What makes a man a religious man is self-denial. If he has learned the lesson that he must put aside his own good and happiness and think only of the good of others, then we may say he is a religious man, and offer him with all our hearts the right hand of fellowship without asking any question as to the form of words or of outward service in which he expresses his belief.

'I have made these observations in order to convey to you my feeling that it is the earnest doer and not the earnest talker that we want. There is evil enough in the world—evil enough in our lives to fight against—good enough, too, all about us to ally ourselves to. It is a good thing that we should associate ourselves formally for these objects, and on this ground I cordially invite those of you who are here for the first time to join with us.'

The impression made by Burdon Sanderson on others is shown by the following notes made by a friend of later days, Professor Hume Brown, Historiographer-Royal for Scotland:

'My recollections of Sir John Burdon Sanderson are chiefly associated with country walks when he was on

holiday, or when the state of his health had compelled him to desist from his scientific work. On such occasions he became expansive to a degree unusual with him, and he hardly, if ever, made reference to his own special studies. What struck one in converse with him was his extreme intellectual modesty. Like Henry Sidgwick, he talked to younger men as if they were his equals, and his deference to their opinions was even embarrassing. What seemed to give him special pleasure was to ascertain what were *their* aims and outlook. His own views regarding the deeper problems of life he apparently shrank from communicating, and this reticence was characteristic of his whole nature. What was said of the poet Gray—*that he never spoke out*, never gave utterance to his deepest self—was singularly applicable to Burdon Sanderson. Keenly sensitive by nature, he concealed his feelings under a reserve which might easily be mistaken for coldness of temperament, but which was fully understood by those who really knew him. In the expression of his opinions he showed the same restraint. Nothing short of the exactest truth would satisfy him, and this was, perhaps, the reason why he could never have been a popularizer of science. He was a man apart from other men by his rare distinction of character, by his keen-edged intellect, and his complete devotion to the work to which he had given his life. Such a character and such a life naturally inspired reverence and affection, and with these feelings he was regarded by all who were admitted to his intimacy.'

Travelling was one of Burdon Sanderson's greatest relaxations—indeed, it might almost be said to have been his only relaxation. Nearly every year a month was spent in this way, and usually in investigating some out-of-the-way part of the country. His wife was his constant companion in all these expeditions. Switzerland was visited in days when Switzerland was not the popular resort it now is, and Italy, Germany, Austria, Sweden, France, and Egypt also. No definite plans were made beforehand, but it was settled from day to day what should be the next halting-place. There were years in which the holiday was shorter and was taken in Great Britain—in Wales, Cornwall, or more frequently in Scotland, where a stay was always made with

his married sister in Perthshire. A driving tour of a few days' duration was also a favourite form of holiday. In 1887 a three weeks' visit was paid to St. Andrews, where work was carried on at the Marine Laboratory there, and several much-enjoyed stays were made at different times with Mr. and Mrs. Romanes, friends of many years' standing, in Ross-shire; these visits were usually combined with work on scientific subjects of interest to host and guest alike. In later years he often spent part of his holiday with Professor and Mrs. Wyndham Dunstan, who were much-valued friends and frequent visitors.

On these holiday expeditions, even in later life, Sanderson seemed to become young again. Few people had more thorough enjoyment of Nature and natural beauty than he. Every natural object was of interest to him. He would throw his whole soul into the construction and correct orientation of a sundial; he would talk to the shepherds and country folk about their daily pursuits and avocations; he discovered curious stones and mosses, and always had his pocket magnifying-glass ready to examine them. Every bird seemed known to him as well as its particular note, and an unnamed flower was always brought home for investigation. His walks extended to many miles, even when age crept upon him, and as the tall thin figure strode along with soft felt wide-awake hat, Scotch plaid across the shoulders, and stick in hand, every passer-by turned to gaze at him. The plaid served other purposes besides that of covering, for it was used in order to negotiate the barbed-wire fences that obstructed a course which never took account of walls or hedges. The talks in the course of these long walks on science and philosophy, about old days in the Cheviots or in Edinburgh closes, can never be forgotten by those who shared in them. One of Wordsworth's verses, which Burdon Sanderson himself wrote on the fly-leaf of his diary, expresses his views of Nature, and the manner in which natural objects affected him, with much truth and beauty:

'My heart leaps up when I behold
A rainbow in the sky;
So was it when my life began,
So is it now I am a man,
So be it when I shall grow old,
Or let me die.'

Music was always another great delight to Burdon Sanderson; he enjoyed a concert exceedingly, but even more he enjoyed hearing classical music played at home. In early days he had little time for general reading, but as life advanced and his life became a less strenuous one—or, at least, one with more outside interests than of old—he read much more in every department of literature.

Burdon Sanderson frequently attended the annual meetings of the British Association, when he much enjoyed meeting his scientific friends. Between 1872 and 1893 he was several times President or Vice-President of the Biological Section, and in 1893 he was President of the Association. Among his most interesting writings are the presidential addresses which he delivered, two of which are reprinted in this volume. In these addresses, and in two others which are reprinted, he discussed some of the questions which are of most fundamental importance in his own science of Physiology.

Burdon Sanderson stoutly maintained the thesis that vital processes, in so far as they can be scientifically investigated, must be regarded as in ultimate analysis nothing but physical and chemical processes, and that the only true method of advance in Physiology is the application of physical and chemical methods of investigation and explanation to physiological phenomena. He was in agreement with other physiological leaders in this contention; but very few had so firm a grasp of its importance or perceived so clearly the difficulties which it involved.

Up to about the middle of last century the prevailing opinion had been that the processes of life involve something different in kind from inorganic phenomena; and to this distinguishing factor the name 'vital force' or 'vital

principle' was given. All the more distinguished younger physiologists of that time—particularly Du Bois Reymond, Helmholtz, Ludwig, and Claude Bernard—revolted against this 'vitalism', and set before themselves the aim of explaining organic on precisely the same general principles as inorganic phenomena. For a long time this movement, which found a brilliant English literary exponent in Huxley, seemed to be carrying everything before it; but gradually it became evident that the problem of reaching physico-chemical explanations of the processes of life is far more difficult than was at first supposed. By degrees it was realized that each constituent cell in a living organism is itself a living organism, and that 'cell activity' is almost everywhere the determinating factor in physiological processes which had been suspected to be capable of a more or less simple physical or chemical explanation. The more the activities of living cells were studied, the more complex and at the same time definite were these activities found to be. As a consequence many physiologists began to use language which implied, or seemed to imply, doubts as to the possibility of ever reaching physico-chemical explanations of fundamental organic processes: and even if they did not admit that they were returning to the old vitalism, they began to distinguish 'vital' activity from the physical and chemical processes which occur within the living body. A few went further, and definitely maintained that the attempt to reach ultimate physical and chemical explanations of distinctively organic phenomena is wrong in principle.

As will be seen from the papers reprinted in this book, Burdon Sanderson was a firm upholder of the view that all the processes which a physiologist can investigate are in ultimate analysis physical and chemical in nature, and that the only fruitful method of advance in physiology is to apply to the processes of life the same principles of investigation and explanation as have been successfully applied in the inorganic world. He refers to one after another of

the cases on which the 'neo-vitalists' base their contention, and endeavours to point out the proper method of dealing with these cases.

'It is perfectly true', he remarks, 'that living cells, like working bees, are both the architects of the hive and the sources of its activities; but if we ask how honey is made it is no answer to say that the bees make it. We do not require to be told that cells have to do with the making of lymph, as with every process in the animal organism; but what we want to know is *how* they work, and to this we shall never get an answer so long as we content ourselves with merely explaining one unknown thing by another. The action of cells must be explained, if explained at all, by the same method of comparison with physical or chemical analogues that we employ in the investigation of organs.'

He looked in particular to the development of knowledge with regard to intra-cellular ferments or enzymes for the solution of difficulties in the physico-chemical interpretation of cell-activity. Thus in his Address on 'The Cellular Pathology of To-day' he says, with special reference to the researches of M. Duclaux:

'In the domain of micro-biology the enzyme may in a certain sense be said to have dethroned the cell. For if, as M. Duclaux continues, we can extract from the cell a substance which breathes for it, another which elaborates the simple from the complex, and finally another which reconstitutes the complex from the simple, the cell can no longer be considered as one, but rather as a complicated machine, the working of which is for the most part dependent on enzymes, which, however numerous and varied may be the processes in which they are engaged, all follow and obey the universal law of adaptation, and all contribute to the welfare and protection of the organism.'

In his Address to the British Association of 1889 he clearly pointed out that this intra-cellular mechanism is far beyond the reach of direct anatomical or microscopical examination, and that it is only by experimental analysis of function that we can infer the nature of the structure on which function depends.

In connexion with the activities of the central nervous system he apparently made no exception in the application of the same general principles, although he clearly pointed out that Physiology can throw no light on the consciousness which may accompany processes in the nervous system. The following passage from his Introductory Public Lecture at Oxford states his position on this point:

'The effort to explain even the most complicated acts as manifestations of the reaction of a living machinery to influences from outside, involving as it does an intimate knowledge of the structure and functions of that living mechanism and all its parts, is one to which some of our best workers are giving some of their best work; but none of these men commits the gross error of supposing that the psychical concomitants of the excitatory process in the brain are things which our methods can help to explain. Towards the problem of the nature of these concomitants we can contribute nothing, simply because they are not things which we can compare with any standards we possess. All that we can do about them is to localize them; but in accomplishing this we are well aware that our researches neither help nor hinder us in the endeavour to penetrate the mystery of our own existence. All this is so plain that it would appear superfluous to state it, were there not persons who need to be informed on the subject; persons who imagine that because our method is founded on the assumption that every material process is the product of material influences, every measurable effect the product of measurable causes, we extend the method to things beyond our province, namely, to things which cannot be measured. A physiologist may be a philosopher if he has the gift for it, but from the moment that he enters the field of philosophy he leaves his tools behind him.'

There can be no doubt that in his views as to the scope and results of Physiology Burdon Sanderson expressed what was in the minds of the ablest physiologists of his time. His clear thinking and dignified language marked him out unmistakably as a leader. It may still be asked, however, whether his conclusions will appear to be con-

vincing to those outside his own science, and whether they even afford a satisfactory working hypothesis for mere experimental physiology.

As regards the first question, the assumption that the actions of men and animals are due to the reactions of living 'machinery' within the body is very far-reaching, and evidently implies a definite answer to questions which would certainly be generally held to come within the scope of Philosophy. If Physiology teaches that human action is dependent simply on physical and chemical conditions within and without the body, consciousness being a mere accompaniment, however mysterious, of certain processes in the central nervous system, such teaching comes into sharp conflict with the results of philosophical investigation, and will never be generally accepted without cogent proof, such as certainly does not at present exist. Probably, however, Burdon Sanderson did not intend to convey as much as his words sometimes appear to imply. No such conflict arises if it is merely maintained that Physiology can only investigate those bodily processes which are physical or chemical in their nature. In his Address as President of the British Association—an Address of which the exact significance was at the time much discussed—he seemed indeed to take up the position that the phenomena of life include something which separates them from physical and chemical phenomena, namely, the presence everywhere of what, following Treviranus, he called 'adaptation'. In summing up he says:

'What I have desired to insist on is that *organism* is a fact which encounters the biologist at every step in his investigations: that in referring it to any general biological principle, such as adaptation, we are only referring it to itself: that no explanation will be attainable until the conditions of its coming into existence can be subjected to experimental investigation so as to correlate them with those of processes of the non-living world.'

It is not easy to harmonize this and one or two further

passages in the same Address with his other published expressions of opinion, both earlier and later, on the same subjects.

Some manuscript notes which he made on reading *The Pathway to Reality*, the Gifford Lectures of 1902-3, by his nephew, Mr R. B. Haldane, may also be quoted. Referring to a sentence on page 242 of this book, in which it is asserted that 'science is making no progress towards the exhibition of life as a specimen of mechanical or chemical action', he writes: 'This I can accept because no such known action or system of actions results in life.' But he disagrees with the author's objections to regarding mechanical and chemical principles as the basis of Biology, and remarks that 'all discovery in Biology is the discovery of the operation, where before concealed, of mechanical and chemical principles.'

The following interesting passage is from a letter which he wrote from Algiers in April, 1904, to Miss Florence Buchanan, in connexion with the paper referred to on page 145:

'From your pencil notes on my MS. I take it that you regard as the *result* of an investigation of the excitatory process the complex of data relating to localization, time-relations, and intensity of electrical change—all of these being measurements. To me it appears that when you have got by measurement a complete knowledge of what happens electrically (intensity, localization, and time-relations), this knowledge, however exact it may be, is of no value unless it enables you to conjecture the nature of the *excitatory process* of which these phenomena are the concomitants.

'The excitatory process can best be defined as a sudden transition from less functional activity (the so-called rest-state) to greater. It is not a measureable physical change but a vital one which cannot be measured, and which *therefore* lies outside the scope of scientific knowledge. The two acts which seem to constitute the excitatory process viz. excitation and response, are not continuous, but are joined together by a non-measureable link. This link is

a subject of scientific conjecture, not of scientific knowledge; for nothing that is not measureable is known. It is, in short, something which is involved in *organism*, for which the most appropriate designation is *organismal*.

'The point to be emphasized is that the organismal link or nexus is the *essential* part of the excitatory process; for neither the physical effect of the stimulus nor that of the response is effectual by itself. It is only when these two are coupled by the organismal nexus that the excitatory process is constituted.

'The propagation of the excitatory process thus constituted takes place, not through or by any measurable process, but is wholly and solely organismal, and therefore not measureable. The electrical machinulae are acted on by the organismal stuff and not by their neighbours. Propagation is a vital process, not a physical one.

'The purpose of my paper will be (in case it is ever written) to show (1) that the mere statement of measurable data stops short of its purpose because it misses the essential factor in the excitatory process; (2) that every electrical change accompanying excitation which is cyclical corresponds to a single organismal change; (3) that the organismal change is modified by (*a*) exhaustion, and (*b*) injury, these being localized, (*a*) at the proximal contact and (*b*) at the distal, and having opposite signs.' (Here of course the ligatured muscle preparation is referred to.)

The paper sketched out in this letter would have been of the highest scientific interest had Burdon Sanderson lived to complete it; but unfortunately increasing illness prevented him. It is clear from the letter that he had come to the conclusion that physiological processes involve something which is neither physical nor chemical in nature, and which is not a mere mysterious accompaniment of these processes, but which is their 'essential part'. He still, however, held to the opinion that it is only the physical and chemical links in the chain of physiological process which can be scientifically studied and known, and he never seems to have modified the opinion he so often expressed that Physiology only came into existence as a distinct branch of science when the

leading physiologists of the preceding generation definitely abandoned vitalism and introduced purely physical and chemical methods of experiment and explanation. To him this appeared to be an 'abandonment of theory for fact, of speculation for experiment'. Those who are unable to agree with him may well point out, however, that from the time of Descartes onwards to the present day the path of physiological advance is strown with the fragments of shattered mechanistic theories, and that at no time has the apparent contrast between the living and the not-living stood out more clearly than at present. Speculation seems always to be necessarily associated with scientific work, and it is hard even to state the results of an experiment except in terms which imply more or less of speculative interpretation.

Is all that can be known and understood of that 'invisible mechanism of life' towards which Burdon Sanderson was striving to penetrate, nothing but physical and chemical mechanism? The nearer we approach it the more complex does the supposed mechanism appear to become, and the more difficult does it seem to form even the remotest conception of how this marvellous 'mechanism' not only maintains itself during life, but reproduces itself from generation to generation. The problem of unravelling the details of such a mechanism is one in comparison with which other problems of Chemistry and Physics would be mere child's play. Are the conceptions with which the sciences of Physics and Chemistry have hitherto furnished us adequate to the solution of such a problem? Do they seem, even, to be bringing us any nearer a solution? It may be long before agreement is reached on these great questions; but in any case Burdon Sanderson's contributions to the discussion will retain their interest and historic value, and will perhaps transmit to many who never knew him something of the spirit which inspired his life.

No memoir of Sir John Burdon Sanderson, however short,

would be complete without the mention of the wife to whom he owed so much, and with whom fifty-two years of perfect happiness were passed. Ghetal Herschell was born in 1832, and was the daughter of the Rev. Ridley Herschell. Her mother was Helen Mowbray, the daughter of a Scottish merchant of well-known family. Mr. Herschell was of Jewish origin, and his parents were natives of West Prussia. He was converted to Christianity comparatively early in life, and after a time of great stress and difficulty he devoted himself to preaching both to his own people and to others. His addresses were very remarkable in character, and they produced a profound effect on those who heard them. Mrs. Herschell was a woman of great culture, and as her marriage was not popular with her family, she had many difficulties to contend with—difficulties which she, however, completely overcame. The Herschells had a large circle of warm friends, and the children had a bright and lively home. The girls were in great degree taught by their mother, who proved an excellent instructress, and both were highly educated women. The younger sister, who married Mr. John Cunliffe, survived her husband, and died in her sister's house in 1899. The brothers were both exceptionally gifted; one, who was much loved in the family, Ridley, died while still quite young; the other, Farrer, afterwards Lord Herschell, became Lord Chancellor of England.

Ghetal married in 1853 at the age of twenty-one. A more devoted and helpful wife she could hardly have been; and in addition to that devotion she encouraged her husband in his work in every way in her power. But for her Burdon Sanderson would often have sunk into those fits of depression to which he was subject, and from which his wife, with a mixture of sympathy and mild raillery, constantly roused him. She wrote for him, played and sang to him (for, like her mother, she was devoted to music), walked with him, and read to him. Indeed, he seemed never happy when she was not near him, and in later days this

was specially evident. After his death she was engaged in writing the record of his life, but the completion of her work was prevented by an accident which eventually caused her death. While she was visiting Aberdeen on the occasion of the meeting of the National Union of Women Workers, one of the many philanthropic societies in which she was interested, she was knocked down by a station trolley and was seriously hurt. She partially recovered from this accident, and displayed great courage during her illness, but about three months afterwards she had an attack of cerebral haemorrhage at the house of her husband's niece near Aberdeen, and a renewed attack some months later at Banchory, to which she had moved, caused her death. She did much for Somerville College at Oxford, and for many educational and charitable objects, and in early days she wrote short memoirs of her parents ; but her greatest work was the love and support which she gave so fully and ungrudgingly to her distinguished husband.

II. PAPERS AND ADDRESSES

THE EXCITABILITY OF PLANTS[1]

Part I. ANIMAL EXCITABILITY.

The subject which I have to bring before you this evening may be best defined as relating to one of the essential endowments of protoplasm, i.e. of the living material out of which the forms of animal and plant life are moulded. It is one which we associate rather with the nature of animals than with that of plants, though it is common to both. In every kind of living matter we are able to observe an alternation between quiescence and activity; and that the transition from the former to the latter is determined by external influences, in other words, excited by stimuli. But in general we confine the term *excitability* to those cases in which the transition is sudden and obvious, and particularly to those in which it is attended or followed by visible changes of form of the excited parts.

When, in 1874, I had the honour of giving an address on a subject included in the present, I had to announce what was then a new discovery, namely, that a phenomenon which had been long known to be characteristic of the transition from rest to action in animal protoplasm also manifested itself in the plant. In all animal structures which are excitable or irritable, i.e. which possess the property of being suddenly called into action when excited, it is found that the waking up—the transition from rest to action—is attended by an electrical disturbance which is of short duration, precedes action, and follows excitation.

It is well known that there are parts of plants which show the same kind of 'wakeableness'—which pass suddenly from quiescence into motion when stimulated; but until 1873 the question had not been asked whether, here also, the going into action is attended with electrical change?

In consequence of a suggestion made by Mr. Darwin one summer morning in that year, that if such were found

[1] A Paper read to the Royal Institution of Great Britain, June 9, 1882.

to be the case it would afford strong confirmation of the view to which he had been led by entirely different considerations, of the close relation which subsists between the essential vital processes of plants and animals, I undertook at once to examine into the subject. It was the rough result of that inquiry that I brought before you in 1874. What we expected to observe was observed. It was found that in the leaf of *Dionaea muscipula*, which was selected as the example of plant-excitability best adapted for the purpose, the touching of the sensitive hairs was immediately followed by an electrical disturbance, which preceded the visible motion of the leaf. As the electrical phenomena observed strikingly resembled those which present themselves under similar conditions in animals, there seemed no room for doubt that the analogy which had suggested the discovery was a true one. But in 1876, Professor Munk, of Berlin, an animal physiologist of the highest reputation, published an elaborate paper on Dionaea, in which, while he admitted that the facts which had been recorded were in the main true, and that a real relation existed between the electrical disturbance which follows excitation in Dionaea and the so-called 'negative variation' of animal physiology, he charged me with having entirely misinterpreted and misunderstood that relation; and in 1877 another still more important research was published by Dr. Kunkel, in which the question was approached from the side of plant-physiology. Professor Kunkel's experiments related not to Dionaea but to Mimosa. His conclusions were as directly opposed to those of Dr. Munk as they were to mine; for his main position was that all electromotive phenomena observed in the organs of plants, are dependent on changes in the distribution of water in their tissues, and consequently have nothing whatever in common with the electromotive phenomena of muscle and nerve. Even had there not been other good reasons for resuming the investigation of the subject, this contradiction of opinion would have rendered it necessary.

Every one who contemplates the behaviour of the sensitive plant, or of the Flytrap, is led to exclaim: If it had but nerves we could understand it! Let us for a moment inquire what there is in nerve which these animal-plants seem to require. The question is easily answered. Nerve is the channel by which, in the animal

body, the influence of any change which takes place in one part of the organism is conveyed to other parts at a distance, independently of the transmission of any sensible motion. Haller, who is well called the father of physiology, sought to explain the propagation of influence in nerve, from the organ of the will to the muscles which it governs, by the transmission of motion of liquid contained in a tube (as in this little apparatus in which my hand represents the will, the long flexible tube the nerve, and the indicator at its farther end the muscle). It is more than a century since Haller made this comparison, but even then he was behind his time, for a greater than he—Newton—had clearly recognized that the process by which the brain becomes cognizant of what goes on at the surface of the body cannot be attributed to the communication of any visible or sensible motion. Newton, although at that time no one had ever seen nerve-fibres as we now see them under the microscope, yet described them with perfect truth as 'pellucid and uniform hair-like filaments', in which vibratory motion could be propagated.[1] This conception Haller, who was certainly not wanting in imagination, rejected. Failing to understand that the thrills which Newton contemplated were of an order far more subtile than those of sound, he argued that, if the function of nerve were dependent on the propagation of vibratory motion, these would so interfere one with another, that all distinctness of impression and of action would be lost, &c. Haller's doctrine of the nerve-fluid held undisputed sway for a century; we have still traces of it in the language used by medical writers. But the notions which we now entertain on nerve-function are much more allied to those of Newton—so like, indeed, that they might be clothed in his language.

The transmission of an impression, i.e. of a state of excitation in a nerve, has been justly compared to the propagation of a mechanical disturbance along a row of card houses, so arranged that the collapse of any one of them necessarily determines that of its neighbours. Such a structure exhibits the properties assigned by Newton to the pellucid capillamenta of nerves, its card houses corresponding to the particles of which the pellucid substance was conceived by him to be made up. In the

[1] See Query 24 at the end of the Third Book of Newton's *Optics*, Dr. Horsley's edition, 1782, p. 226.

one case, as in the other, a disturbance (excitation) which originates at any point is propagated in either direction, reaching its goal in a time which is proportional to the distance travelled.

A still better illustration is furnished by the propagation of an explosion. Here, for example, is a train of gun-cotton.[1] When I excite the end of the nerve with a match, a blaze runs along to the opposite end, of which you can easily trace the progress, and if, in repeating the experiment, I partially block the explosion by compressing the strand midway by a weight, you see plainly enough that propagation is retarded at the obstacle.

The main ground for the statement I have made to you, that the transmission of an impression along a nerve is analogous to the propagation of an explosion, lies in the proof first given by Helmholtz, that time is lost in the transmission of excitatory effects along nerves, and that the time is proportional to the distance. This I will endeavour to illustrate by an experiment. Tracing the motor-nerve channels by which, in the human body, the influence of the will is conveyed to the muscles of the thumb and finger as in the act of pinching, descriptive anatomy teaches me that these approach the surface sufficiently closely to be within reach of induction currents led through the skin, at two places, namely, above the collar-bone and at the bend of the elbow. If, by the means I have indicated, I excite the nerve at either of these points, the hand involuntarily pinches, but if I measure the time which elapses in the two cases between the excitation and the muscular response, I find it to be different—the difference being the measure of the time occupied by the excitatory change, from the clavicle to the bend of the arm.[2]

The experiment you have seen not only serves to illus-

[1] Mr. Abel, with the greatest kindness, enabled me to illustrate this part of the lecture by experiments, showing how, according to the nature of the explosive substance, the velocity of propagation is very different, though the mode of propagation is the same.

[2] The measurement is effected by recording the muscular action on a blackened glass plate, which is so fixed to a pendulum that its surface is parallel with the plane of oscillation. The pendulum is allowed to make a single swing from right to left, and in doing so strikes a trigger, the effect of which is to excite the nerve by an induction shock at one or other of the points indicated. Two experiments were made at the lecture in immediate succession, in one of which the nerve was excited at the more distant, in the other at the nearer point. The difference of

trate the resemblance between the propagation of the excitatory state and that of an explosion, but also to exhibit the contrast between them. It is common to all excitable structures, whether of plant or animal, that, provided the intervals are not too short, the excitations can be repeated any number of times without losing their effect, a fact which can only be explained on the hypothesis that in such structures provision exists for the immediate recuperation of lost energy ; or, in other words, that the machinulae which take part in the propagation of the excitatory disturbance are endowed with the faculty of quickly recovering their original condition, so as to be ready for another excitation.[1]

In bringing before you these elementary facts relating to animal excitability, my object is to use them in the comparison we shall shortly have to make between animals and plants in respect of this property. From the last experiment we have learnt, in addition to the fact which it was specially intended to illustrate, that the sudden change of form of a muscle which we call its contraction is of such a nature that the structure shortens in one direction only, and that it gains in thickness in the same proportion that it diminishes in length. The experiment further illustrates the fact that a muscle does not contract of itself, but that it undergoes this change only when it is directly or mediately excited. Bearing these facts in mind, I would ask your attention to some further characteristics of the excitatory process in muscle, a knowledge of which is essential to our purpose.

The first of these is, that in every such process the visible response (in the case of muscle the contraction) is separated in time from its cause, the excitation, by a period during which no visible change occurs, although, for reasons which I need not here insist on, molecular changes must be in progress. With suitable appliances

time between the two records, calculated from the distance from each other of the two tracings, was about $\frac{1}{50}$ of a second. As the distance was about 13 inches, this gives about 66 metres per second as the rate of transmission. This result was probably not far from the truth, but the reader will understand that a measurement made under the conditions of a lecture experiment could not be relied on.

[1] The extreme shortness of the interval of time between successive excitations of muscle was illustrated by an experiment in which the rheoscopic limb of a frog was kept in repeated spasmodic action by the voice of the lecturer acting on a telephone of which the wires were in contact with the nerve.

it would not have been difficult to prove this to you experimentally in respect of ordinary muscle, but it can be much more easily demonstrated if I substitute for ordinary voluntary muscle, the muscular tissue of the heart, in which the process is about fifteen times as slow. Here, although the excitatory changes occur in the same order, and are of the same nature as in common muscle, the interval between excitation and response, amounting to about a sixth of a second, is very easily perceived.[1]

It is obvious that this interval may be regarded as a period of transition from the quiescent to the active state, and I told you at the beginning of the lecture that it was always accompanied by electrical changes of a characteristic kind in the excited part. I wish now to show you, that in the muscular substance of the ventricle of the heart, in which we have been able to observe the existence of an interval of apparent inactivity between excitation and visible response, this transition-time is occupied in the way that has been stated. For this purpose we have arranged the ventricle of the heart of a frog in such a way that it can be projected on the screen. At the same time the surface of the ventricle is led off to the galvanometer, the electrodes being applied one at the base the other at the apex. The galvanometer is so arranged that the image is thrown on the screen close to the lever. On exciting the heart as near as possible to the apex, the image shoots off in a direction which indicates that the excited part of the surface of the ventricle becomes negative to the rest, and it is seen at the same time with perfect distinctness that the electrical effect precedes the mechanical, i.e. the rise of the lever.

There are two other facts which are of importance for our purpose, and for the demonstration of which the muscular tissue of the ventricle of the heart of the frog is also available. The first is that during a certain period after each excitation, which M. Marey has called the 'refractory period', but which is more correctly termed the period of diminished excitability, the tissue does not respond to a second excitation: the second is, that the

[1] For this purpose the conical ventricle of a frog's heart was projected on the screen with a weight attached to its apex, the base being fixed. It was excited directly by an induction shock, an electro-magnetic indicator, interpolated in the primary circuit of the inductorium being also projected. The interval of time between the two events, viz. the induction shock and the response, was made obvious.

duration of the excitatory effect (as indicated by that of the electrical disturbance of the mechanical effect which follows it, and of the state of suspended excitability) is governed by the temperature at which the observation is made. The first of these propositions may be illustrated by arranging an experiment in such a manner that a ventricle at 10° C. receives two excitations (induction shocks) at an interval of about a second. Both are effectual, but, if the interval is in the slightest degree shortened, the second fails, for it falls within the period of suspended excitability. The proof of the second proposition can of course only be obtained by series of measurements of the time occupied respectively by the electrical disturbance and by the contraction, at different temperatures; but when successive observations are taken of the same ventricle at temperatures which differ by several degrees, the contrast is very readily appreciated.[1]

The experiments you have seen this evening may, I trust, have served to illustrate the main facts of animal excitability sufficiently to enable us to proceed to the subject which more specially interests us—that of the excitatory phenomena of plants.

Part II. PLANT-EXCITABILITY.

The number of plants which exhibit what is often called irritability is very considerable. I will not weary you with even enumerating them. You will see from the table that they are distributed among a number of natural orders, so that one might be inclined to suppose that in this respect no relation could be traced between the physiological endowments and the morphological characters of a plant. That it is not so we have abundant evidence. Thus, in the same genus we may find all the species excitable, though not in the same degree. The extreme sensitiveness of the Chinese Oxalis, formerly called *Bio-*

[1] To illustrate the influence of temperature two ventricles were projected on the screen, of which one was in contact with a lacquered metal surface at 10° C. the other at 15° C. In the latter case the time occupied in the contraction was about half a second shorter than in the former. As regards the period of suspended excitability, it was first shown that at 10° C. the second of two excitations was ineffectual, but by raising the temperature two or three degrees, the state of things was so changed that both excitations were followed by a contraction, the refractory period, like that of systole, being shortened by the warming.

phytum sensitivum, because it was supposed to be particularly alive, appears in a less degree, but equally distinctly in our own wood-sorrel, as well as in the Tree Oxalis of Bengal—the Carambola [1], which is described in

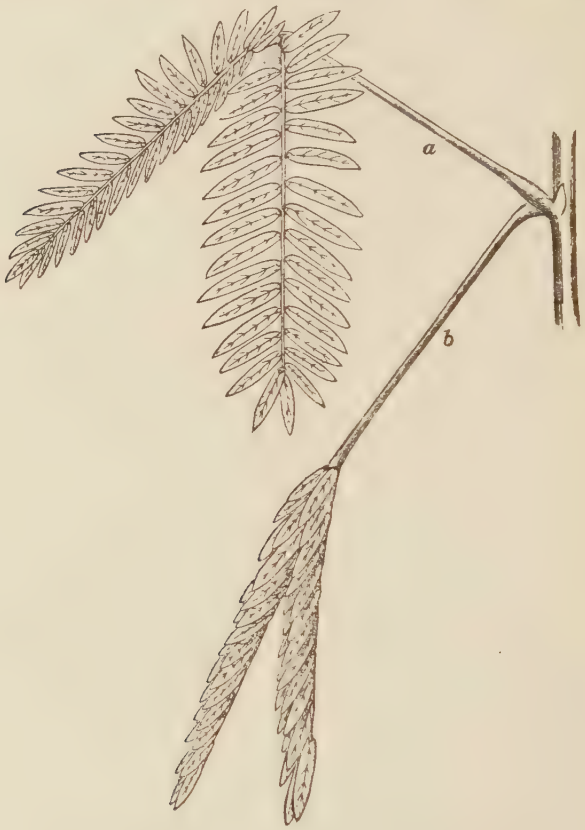

FIG. 1. Leaf of Mimosa: *a*, in the unexcited state; *b*, after excitation (after Pfeffer).

an interesting letter addressed by Dr. Robert Bruce to Sir Jos. Banks, and published in the *Philosophical Transactions*. Again, in the same order, as, for example, among composite plants, we may have the Thistles, Knapweeds, and Hawkweeds, all showing excito-contractility in

[1] 'An account of the Sensitive Quality of the tree Averrhoa Carambola.' By Robert Bruce, M.D. (*Phil. Trans.*, vol. lxxv, p. 356.)

the same way, although the plants do not at all resemble each other in external appearance. In order to make you acquainted with the mechanism by which the excitable motions of plants are brought about, I will confine myself to a very few examples, selecting, of course, those which have been most carefully investigated.

Every one is acquainted with the general aspect of the sensitive plant. Probably, also, most persons have observed the way in which the leaves behave when one of them is touched, namely, that the leaf, instead of being directed upwards, suddenly falls, as if it had lost its power of sup-

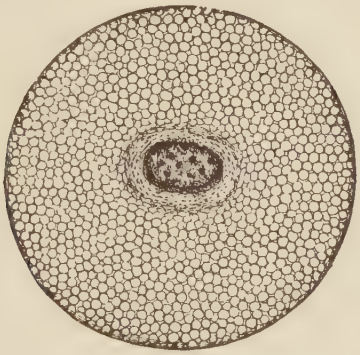

Fig. 2. Section of the motor organ as projected on the screen. The vascular bundle in the middle of the section consists of a cylinder of thick-walled woody fibres and vessels, surrounded by a layer (annular in section) of elongated cells. The parenchyma is thicker below than above the vascular bundle. The section fails to show that the cells of the upper half have thicker walls.

porting itself, and that the little leaflets which spring from the side-stalks fold together upwards (Fig. 1). But perhaps every one has not observed exactly how this motion is accomplished, namely, that by means of little cylindrical organs the leaflets are jointed on to side-stalks, the side-stalk on to the principal stalk, and the principal stalk on to the stem. In those little cylinders, the powers of motion of the leaf have their seat. They may, therefore, be called the motor organs of Mimosa. I would ask your attention to their structure.

In my description I will confine myself to the relatively large joint at the base of the principal leaf-stalk. If you make a section through it in the direction of its length,

you find that it consists of the following parts. In the axis of the cylinder is a fibro-vascular bundle; above it are numerous layers of roundish cells with thick walls, and between these there exist everywhere intercellular spaces, which in the resting—that is the excitable—state of the organ, are filled with air. The surface is covered by epidermis. Below the axial bundle there are equally numerous layers of cells, but they differ from the others in this respect, that their walls are more delicate (Fig. 2). And now let us study the mechanism of the motion. The literature of this subject is voluminous. Substantially, however, we owe the knowledge we possess to two observers—E. Brücke[1], who studied it in 1848, and Pfeffer[2], whose work appeared in 1873. I must content myself with the most rapid summary.

Let me begin by noticing that Mimosa, in common with many other excitable plants, exhibits that remarkable phenomenon which we commonly call the sleep of plants, that is, that as night approaches the leaf-stalks sink, and the leaflets fold up, the whole leaf assuming a position closely resembling that which it assumes when it is irritated. All that time will allow me to say on this subject is that although the leaf assumes the same position in sleep as after excitation, the two effects are not identical. The state of sleep differs from that in which the plant finds itself after it has been irritated in two particulars. The first is, that in the state of sleep it is still excitable, and responds to stimulation exactly in the same way, although from being already depressed the extent of its motion is diminished; the other is, that in sleep, the joint, although bent downwards, is still more or less resistent and elastic; whereas in the unexcitable (or, what comes to the same thing *excited*) state, all elasticity has disappeared. In a word, in the motor organ of Mimosa, in common with all other excitable structures, the characteristic of the excited state is *limpness*. All the Mimosa plants on the table are in the state of sleep, but are still excitable, for when they are touched they sink to an even lower position than that of sleep, and at the same time become limp. Hence you have, as the result of excitation, two changes, namely (1) the change of position, only to be observed

[1] Brücke, 'Ueber die Bewegung der Mimosa pudica.' Müller's *Archiv*, 1848, p. 434.

[2] Pfeffer, *Physiologische Untersuchungen*, p. 9.

when the plant is awake, and (2) the loss of stiffness, dependent, as we shall see, on a vital change in the protoplasm of the cells, which is also observed when the plant is asleep.

So much for the general nature of the excitatory change. How do we discover what the mechanism is by which this remarkable organ of motion acts? By a mode of experiment which is well known to the physiologist. It may be called the method of ablation. We have here a mechanism which consists of several distinct parts, each, we may presume, having a distinct purpose; and the only method which will enable us to discover what these several purposes are is to observe how each acts alone—or, on the other hand, how the rest act after it has been taken away.

To prove that the motion of the whole leaf is dependent on the motor organ at the base of its stalk, requires no experiment. We see that the leaf descends, the joint bends, while the stalk remains rigid, and we know from its structure that the latter contains no mechanism by which it can act mechanically on the joint, as I act on my wrist by the muscles of my fore-arm.

The question therefore is—What part of the joint is essential? We begin by taking away the upper half, leaving the axial bundle and the lower half, and find that the leaf assumes a higher position than before. When touched, it falls. The function of the upper part, therefore, is merely auxiliary. The essential part is the lower, which in the unexcited state is capable of bearing the weight of the leaf. When it is excited it suddenly becomes weak, and the leaf falls. How does it do this? We will proceed to remove the axial bundle. The cellular cushion expands and lengthens, showing that it is elastic, and has a tendency to spring out when liberated. We have seen that this resistent cushion consists of cells, that is, of little bladders, each of which is distended with liquid; and its tendency to expand as a whole is due to the tendency to expand of the innumerable cells of which it is made up. In the unmutilated state, these are squeezed into a smaller space than that which they would assume if they were left to themselves; and, consequently, as their expansion is prevented, or curbed on one side, it acts on the opposite side, so as to bend the cylinder in the direction of the restraint.

All of this we can, perhaps, better understand by a model; and it is possible to make one which, not only in form, but

in principle, corresponds to the living mechanism it is intended to illustrate. In the model the axial bundle is represented by a strip of leather, the innumerable cells of the excitable cushion by an india-rubber bag. By a pump we are able to fill this cell or cushion more or less with fluid, and thus to vary its tension, and you see that if we increase the tension, the stem rises. By diminution it suddenly falls, just as the Mimosa leaf does when irritated.

We have come then to this point—that the reason why the leaf suddenly sinks on excitation is that the cells undergo a sudden diminution of tension or expansion. But our inquiry is not yet terminated. We have still to ask—How is this loss of tension effected? The answer is, by discharge of water. In the unexcited state all these cells are distended or charged with liquid. Suddenly, when the structure is excited, they let out or discharge that liquid, and it finds its way first into the inter-cellular air spaces, and secondly, out of the motor organ altogether. This we know to be a fact by an experiment of Pfeffer's, which must be regarded as one of the most important relating to the mechanism of plants that was ever made. He observed that if the leaf-stalk is cut off from the motor organ, a drop of fluid appears at the cut surface at the moment that the latter bends downwards on excitation, and that in the experiment described just now, in which the upper part of the motor organ is cut off, there is also, so to speak, a sweating of liquid from the cut surface.

We are, therefore, certain that liquid escapes, but why does it escape? That I shall explain farther on, and will now proceed to two other examples. One is a plant which is a great favourite in London, for it is one which flourishes even in London smoke—Mimulus. For our purpose it is good chiefly because its structure is very simple. It is one of those examples in which excitability is associated with the function of fertilization, and inasmuch as this is a very transitory purpose, the property itself is transitory. When the cells of the stigmatic surface are touched they discharge their liquid contents, and consequently become limp. The outer layer of the lip is elastic, and tends to bend inwards. Consequently when the inner cells lose their elastic resilience it is able to act, and the lip bends inwards. In another allied plant, *Goldfussia anisophylla* (Fig. 3), which was described forty years ago by the Belgian naturalist Morren, we have the same mechanism. In this plant, as shown in

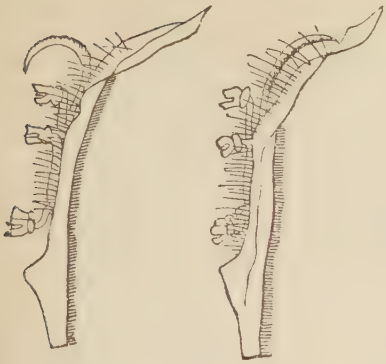

Fig. 3.

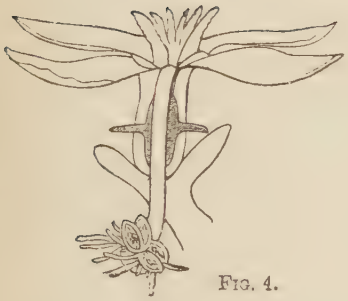

Fig. 4.

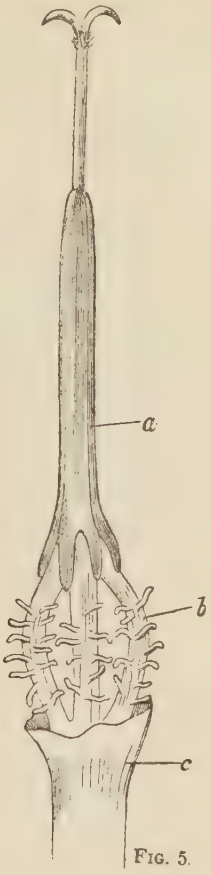

Fig. 5.

Fig. 3. Style, stamens, and part of corolla of Goldfussia. In the left-hand figure the style is in the unexcited state, and is curved upwards, so that the stigmatic surface looks towards the mouth of the corolla. On excitation it suddenly assumes the position shown in the second figure, the stigma looking towards the roots of the collecting hairs.

Fig. 4. Flower of Stylidium, showing the column in the unexcited state, terminating in the anthers and stigma, which are surrounded by conspicuous hairs. It is bent down at the mouth of the corolla, the four principal lobes of which are seen, two on each side, and partly conceals the fifth lobe or labellum.

Fig. 5. A floret of Centaurea as prepared for projection on the screen. The corolla (*c*) has been cut away so as to expose the five filaments (*b*). beset with hairs, and united above into the anther tube (*a*). The filaments are arched outwards, as in the unexcited state.

the drawing, the style is not lipped but awl-shaped. It reaches to the mouth of the showy, orange-coloured corolla, to the inside of which it is united by its under surface. It has a smooth side, the epidermis of which is made up of numerous small prismatic cells and is very elastic, and, in the unexcited state, concave, and a papillated side beset with the nipple-like ends of cylindrical cells, which, when unexcited, are distended with liquid. These cylindrical cells are continuous with those of the conducting tissue of the style. When an insect enters the flower, it does two things: it charges the fringe of hairs on the inside of the corolla with pollen, and touches the style, which, in consequence, bends suddenly in the opposite direction to that in which it was bent before, so as to plunge its stigmatic surface into the fringe. In this motion the epidermis acts as a spring simply. So long as the stigmatic tissue is turgid it cannot act. The moment its cells lose their tension, off it goes.[1]

Another plant investigated by Morren is one of very different organization, but is one in which the existence of excitability has an equally plain teleological interpretation. Long ago Robert Brown, to whom plant-lore owes so much, when exploring the flora of Botany Bay, became acquainted with the now well-known Australian plant called Stylidium.[2] [A specimen from the Royal Gardens at Kew was exhibited.] Here is the plant (Fig. 4). The flower is too small to be easily seen, but the diagram will enable you to understand the mechanism. It has again to do with insects and fertilization. In Stylidium the anthers and stigma are united together at the summit of a cylindrical stem which may be compared with the motor organ of Mimulus. You might naturally suppose that they were arranged so in order that the pollen from these anthers should be at once received by the adjoining stigmatic surface. That it is not so is evident from the order of development of the flower, for you find that at the moment that the anthers burst, the stigma is not yet mature. Consequently the pollen is not intended for it, but for flowers which have come to maturity earlier, and the mechanism which now interests us fulfils this purpose. The figure shows the singular form of this

[1] 'Récherches sur le mouvement, &c., du style du Goldfussia anisophylla.' *Mém. de l'Acad. Royale de Bruxelles*, 1839, vol. xii.

[2] Morren, 'Récherches sur le mouvement et l'anatomie du Stylidium graminifolium.' *Mém. de l'Acad. de Bruxelles*, t. xi, 1838.

strange flower. You observe that the column, as it is called, is bent down over the corolla so as to be in contact with the odd-looking labellum, which here takes the place of one of the petals. At the moment that the anthers burst the column attains its greatest sensitiveness. The slightest touch causes it to spring up, straighten itself suddenly, and then bend over to the opposite side. The mechanism resembles that of Mimosa and of Mimulus. There is a spring, the action of which is restrained by the resilience of cells distended with liquid. Suddenly these cells discharge their contents, and the spring acts.

And now let me pass to another group of plants which may serve as a contrast to Stylidium. Stylidium may be called an out-of-the-way plant. It has an organization which is not represented in the European flora. The family of thistles, and their allies the knapweeds (represented in our gardens by the ladies' blue-bottle), all of which are common wayside plants, exhibit excitable movements which, although of a very different kind from those we have just described, have, like them, to do with the visits of insects for the purpose of fertilization. We will now throw on the screen a single fertile floret of *Centaurea Cyanus* (Fig. 5). The large diagram shows the same floret deprived of its corolla. Its axis is occupied by the style, surrounded by its tube of anthers. Below, the anther-filaments expand into a kind of cage, and again approach one another, when they are united with the tube of the corolla. At the moment that the anthers arrive at maturity these filaments are very excitable. When one of them is touched, it contracts and draws the style towards itself. Immediately afterwards the excitatory effect spreads to the others, all five arches becoming straight and applying themselves closely to the style. A similar effect is produced by an induction shock. [The structure described was projected on the screen ; on passing an induction current through it, the mode of contraction of the filaments was seen.]

The mechanism of Centaurea has been studied by many plant physiologists, particularly by Professor Ferdinand Cohn of Breslau, and more recently with great completeness by Professor Pfeffer. It has in this respect a greater interest than any other—that the shortening of these filaments in response to excitation strikingly resembles muscular contraction. You have here a structure in the form of a flattened cylinder which resembles many muscles

in form, the length of which is diminished by about a sixth on excitation. This superficial resemblance between the two actions makes it the more easy to appreciate the differences.

Let me draw your attention to the diagram of an experiment made last year, which was intended to illustrate the nature of muscular contraction, and particularly to show that when a muscle contracts, it does not diminish in volume. The first difference between muscle and plant is a difference in the degree of shortening. A muscle shortens by something like a third of its length, the anther filament only by a sixth. But it is much more important to notice that in contracting, the filaments do not retain their volume. In shortening, they broaden, but the broadening is scarcely measurable ; hence they must necessarily diminish in bulk, and this shrinkage takes place, as Pfeffer has shown, exactly in the same manner as that in which the excitable cushion of Mimosa shrinks, namely, by the discharge of liquid from its cells.

We are now in a position to study more closely the question to which I referred a few minutes ago—How do the cells discharge their contents? The structure of the filament of Centaurea, from its extreme simplicity, is a better subject of investigation with reference to this question, than any other. Each filament is a ribbon consisting of (1) a single fibro-vascular bundle, (2) delicate cells of regular cylindrical form, (3) an epidermis of somewhat thick-walled cells. [Microscopical preparations were shown.] In Mimosa we saw that the epidermis and vascular bundle took only a passive part in the production of the motion. Here, the part they play is even less important. Everything depends on the parenchyma, which, when excited, shrinks by discharging its water. Pfeffer proved this by cutting off the anther tube from the filaments, and then observing that on excitation a drop collected on the cut surface, which was reabsorbed as the filament again became arched. It is obvious that if the whole parenchyma discharges its liquid, each cell must do the same, for it is made up entirely of cells. To understand how each cell acts, we have only to consider its structure. Each consists of two parts—an external sac or vesicle, which is of cellulose, and, so long as the cell is in the natural or unexcited state, *over-distended*, so that, by virtue of its elasticity, it presses on the contents with considerable force ; and secondly, of an internal

more actively living membrane of protoplasm, of which the mechanical function is, so long as it is in its active condition, to charge itself fuller and fuller with liquid—the limit to further distension being the elastic envelope in which it is enclosed. In this way the two (the elastic envelope and the protoplasmic lining) are constantly in antagonism, the tendency of the former being towards discharge, that of the latter towards charge. This being so, our explanation of the effect of excitation on the individual cell amounts to this—that the envelope undergoes no change whatever, but that the protoplasm lining suddenly loses its water-absorbing power, so that the elastic force of the envelope at once comes into play and squeezes out the cell-contents. Consequently, although here, as everywhere, the protoplasm is the seat of the primary change, the mechanical agent of the motion is not the protoplasm, but the elastic envelope in which it is enclosed.

The complete knowledge we have gained, from our study of the anther filaments of Centaurea, of the mechanism of the excitable plant-cell, can be applied to every other known example of irrito-contractility in the organs of plants, and particularly to that most remarkable of all such structures, the leaf of *Dionæa muscipula*. Although I described the structure of the leaf just eight years ago in this room, I will occupy a moment in repeating the description. The blade of the leaf is united on to the stalk by a little cylindrical joint. Here are two models, in one of which the leaf is represented in its closed state, in the other in which it is in its unexcited or open state. The leaf is everywhere contractile—that is, excitable by transmission, but not everywhere susceptible of direct excitation—or, in common language, sensitive. It is provided with special organs, of which we do not find the counterpart in any of the plants to which reference has been made, for the reception of external impressions—organs which, from their structure and position, can have no other function.

The action of the leaf to which the plant owes its name and by which it seizes its prey, is, in its general character, too well known to require description. In the shortest possible terms, it is the sudden change of the outer surface of each lobe of the leaf from convex to concave, and at the same time the crossing of the two series of marginal hairs, as the fingers cross when the hands are clasped. What I desire to show with respect to it is, that here also the

agents are individual cells—that is, that the individual elements out of which the whole structure is built are the immediate agents in the production of the movement.

A cross-section of the leaf shows the following facts: If the section be made in the direction of the parallel fibro-vascular bundles which run out from the mid-rib nearly at right angles, and happen to include one of these bundles, it is seen that it consists of three parts, viz. of the fibro-vascular bundle in the middle and equidistant from both borders; of the cylindrical cells of the parenchyma on either side, and of an external and internal epidermis. The external epidermis is smooth and glistening, and its cells possess thicker walls than those on the opposite surface.

The most remarkable feature of the internal surface is, that it possesses the excitable hairs, three on each side, which in Dionaea are the starting-points of the excitatory process whenever it is stimulated by touch, as is normally the case when the leaf is visited by insects; for experiment shows that although the whole of the leaf can be excited either by pressure or by the passage of an induction current, the hairs exclusively are excited by touch. It is therefore of great interest to know their structure and their relation to the excitable cells of the parenchyma, with which they are in so remarkable a relation physiologically. In sections such as that which we will now project on the screen (Fig. 6), it is seen that each hair springs from a cushion which consists of minute nucleated cells inclosed by epidermis; and that if we follow this structure into the depth of the leaf, its central cells gradually become larger, until they are indistinguishable from those of the ordinary parenchyma of the leaf. By these cells it must be admitted that the endowment of excitability is possessed in a higher degree than by the ordinary cells of the parenchyma, so that for a moment one is tempted to assign to them functions corresponding to those of motor centres in animal structures (particularly in the heart). There is, however, no reason for attributing to them endowments which differ in kind from those we have already assigned to the excitable plant-cell.

The fact that the excitable organs are exclusively on the internal surface of the lobe, suggests that although the parenchyma of the inside has apparently the same structure, it has not the same function as that of the outside—that is, that although the cells of the outer layers are just like those

of the inner, they are either not excitable at all, or are so in a much less degree. In this way only can we account for the bending inwards of the lobe. In the unexcited state both layers are equally turgid ; as the effect of excitation the internal layers become limp, the external remaining tense and distended.

I will now endeavour to illustrate the motions of the leaf by projecting them on the screen. Here are several leaves

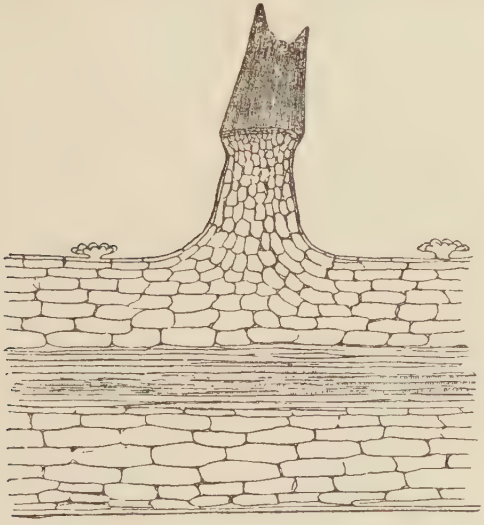

Fig. 6. Transverse section of lobe of leaf of Dionaea comprising the root of a sensitive hair.

which have been prepared by attaching one of the lobes to a cork support ; the other is free, but a very small concave mirror has been attached to its external surface near the margin. The image of the light which falls on the mirror is reflected on the wall behind me. In this way the slightest movement of the lobe is displayed. By this contrivance I wish to show you two things—first, that a very appreciable time elapses between the excitation and the mechanical effect ; and secondly, that when the leaf is subjected to a series of very gentle excitations, the effects accumulate until the leaf closes. This we hope to show by bringing down a camel-hair pencil several times in succession on a

sensitive hair, doing it so deftly that at the first touch the lobe will scarcely move at all. At each successive touch it will bend more than at the preceding one, until you see the lever suddenly rise, indicating that the leaf has closed. The purpose which I have in view is to demonstrate the contrast between the motion of the leaf and muscular contraction. A muscle in contracting acts as one organ—at once. The motion of the leaf is the result of the action of many hundred independent cells, all of which may act together, but may not. In either case they take a great deal longer to think about it; for during a period after excitation, which amounts at ordinary summer temperature to about a second, the leaf remains absolutely motionless.

And now we have to inquire what happens during this period of delay. There are two things which we may assume as certain without further proof, namely, first that something happens—for when I see a certain movement followed after a time invariably by another, I am quite sure that the chain between cause and effect is a continuous one, although the links may be invisible; and secondly, that this invisible change has its seat in the protoplasm of each of the excitable cells.

We have already seen that in muscle this latent state of excitation is not without its concomitant sign—the excitatory electrical disturbance; and I have now to show you that this, which is the sole physical characteristic of the excitatory process in animal tissues, manifests itself with equal constancy and under the same conditions in plants.

It will be unnecessary for my present purpose to enter into any details as to the nature of the electrical change; it will be sufficient to demonstrate with respect to it, first, that when observed under normal physiological conditions, its phenomena are always conformable to certain easily defined characters; secondly, that it culminates before any mechanical effect of excitation is observable, and consequently occupies, for the most part, the period of latent excitation already referred to; and thirdly, that it is transmitted with extreme rapidity from one lobe of the leaf to the opposite. Of these three propositions, it will be convenient to begin with the second. On the left-hand screen is projected the mercurial column of the capillary electrometer of Lippmann. The instrument which we use this evening is one of great sensibility, given me by my friend Professor Lovèn, of Stockholm. The capillary electrometer

possesses a property which for our purpose is invaluable—that of responding instantaneously to electrical changes of extremely short duration. We cannot better illustrate this than by connecting the wires of the telephone with its terminals. If I press in the telephone-plate I produce an instantaneous difference between the terminals in one direction, and in the opposite when I remove the pressure. You see how beautifully the mercurial column responds.

We now proceed to connect the terminals with the opposite sides of a leaf, so that by means of the mirror we can observe the moment at which the leaf begins to close and the first movement of the mercurial column, both being projected on the same screen. We shall see that the mercurial column responds (so to speak) long before the mirror. The difference of time will be about a second.

We now take another leaf, which, with the plants of which it forms part, is contained in this little stove, at a temperature of about 32° C. Our object being to subject the leaf to a succession of excitations, the effect of which would of course be to determine its closure, we prevent this by placing a little beam of dry wood across it, and fixing the ends of the beam with plaster of Paris to the marginal hairs of each lobe. At the same time, wedges of plaster are introduced in the gap between the lobes at either end of the mid-rib. [The leaf so fixed was projected on the screen (Fig. 7).] This having been done, we can excite the leaf any number of times without its moving; and we know that we actually excite it by observing the same electrical effect which, in the first leaf experimented on, preceded the movement of the lobe.

And now I beg you to notice what the nature of the experiment is. The diagram (Fig. 8) shows the position of the electrodes by which the opposite surfaces are connected with the terminals of the electrometer. You will notice that they are applied to opposite points of the internal and external surfaces of the right lobe, and that the left lobe is excited. The experiment consists in this: By the electrodes near *r*, an induction shock passes through the right lobe. Apparently at the same moment the electrometer, which is in relation with the opposite lobe, responds. I say apparently, because in reality we know that the response does not begin until about $\frac{3}{100}$ of a second later. We prove this by a mode of experimentation which is of too delicate a nature to be repeated here. I will explain the mode of

action of the instrument used, by a diagram (Fig. 9) which represents a pendulum in the act of swinging from left to right. As it does so, it opens in succession three keys, of which the first is interpolated in the primary circuit of the

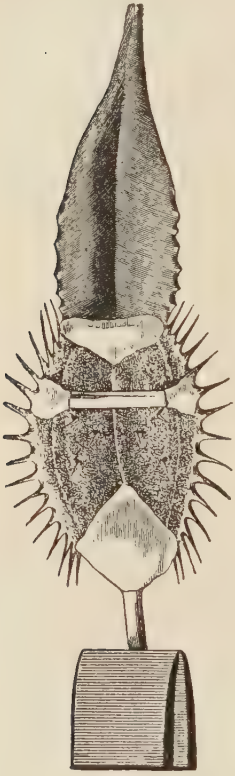

Fig. 7. Dionaea leaf, fixed so as to prevent its closing. (From a photograph.)

Fig. 8. Diagram of ideal transverse section of lamina of leaf of Dionaea. The needle enclosed in a circle represents the electrometer, which, in the experiment described, was substituted for the galvanometer. On the opposite side is shown the secondary coil of the inductorium. *m* is in connexion with the capillary, *f* with the sulphuric acid of the electrometer.

induction apparatus which serves to excite the leaf; the second breaks a derivation wire which short-circuits the electrodes, so that, so long as it is closed, no current passes to the galvanometer, which in this experiment takes the place of the electrometer; while the third breaks the galvanometric circuit. Consequently the opposite surfaces of the leaf are in communication with the galvanometer only between the opening of the second and third keys. These three keys can be placed at any desired distance from one another. If they are so placed that the galvanometer circuit is closed $\frac{1}{100}$ of a second after excitation, and opened $\frac{3}{100}$ of a second, and it is found that there is no effect, it is

certain that the electrical disturbance does not begin at the part of the leaf which is interpolated between the galvanometer electrodes until at least $\frac{3}{100}$ of a second after the excitation. If, on extending the period of closure to $\frac{4}{100}$ of a second, the effect becomes observable, you are certain that the disturbance begins between three and four hundredths of a second after excitation.

By this method we have learnt, first, that even when the seat of excitation is as near as possible to the led-off spot

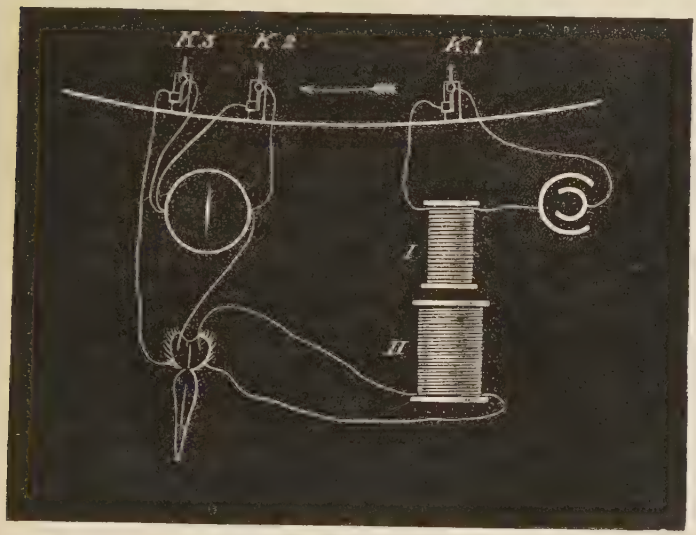

FIG. 9. Diagram of the pendulum-rheotome. K_1, K_2, and K_3 are the keys referred to. I and II represent respectively the primary and secondary coils of the inductorium. The leaf, galvanometer, battery, &c., will be easily recognized.

there is a measurable delay, and secondly, that its duration varies with the distance which the excitation effect has to travel so as to indicate that, in a warm stove, the rate of transmission is something like 200 millimeters per second. It is, consequently, comparable with the rate of transmission of the excitatory electrical disturbance in the heart of the frog.

And now I come to my last point, namely that the electrical change has always the same character under the same conditions. You have already seen that when the

method used is that which I have indicated, the electrical effect consists of two phases, in the first of which the external surface of the leaf becomes negative to the internal. I will now exhibit this in another way. Many present have probably seen in a recent number of *Nature* reproductions of photographs recently taken by M. Marey, of the phases of the flight of birds. If the movement of a bird's wing can be photographed, you will easily imagine that we can also obtain light-pictures of such a movement as that of the electrometer column. You have only to imagine a sensitive plate moving at a uniformly rapid rate

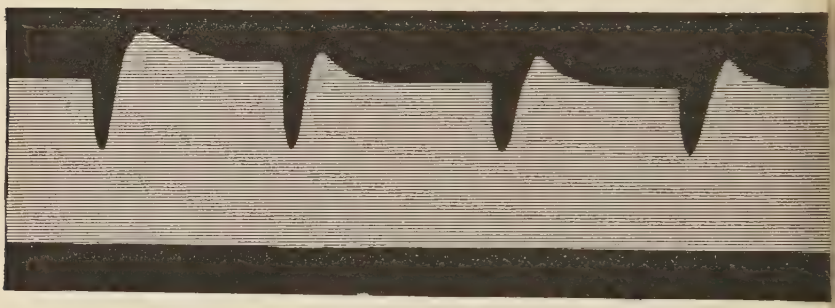

FIG. 10. Copy of photograph of the excursions of the capillary electrometer as projected on a sensitive plate moving at the rate of $\frac{1}{3}$ centimeter per second. The four 'excitatory variations' shown were due to as many touches of a sensitive hair of the lobe opposite to that of which the opposite surfaces were connected with the terminals of the instrument.

taking the place of the screen, and you have as the result the photograph (Fig. 10) which I show. Here are the electrical effects of several successive excitations recorded by light with unerring exactitude. In each, the diphasic character is distinct, and you see that the first or negative phase lasts less than a second, but that the positive, of which the extent is much less, is so prolonged that before it has had time to subside it is cut off by another excitation.

It would have been gratifying to me, had it been possible, to exhibit to you other interesting facts relating to the excitatory process in our leaf. It has, I trust, been made clear to you that the *mechanism* of plant motion is entirely different from that of animal motion. But obvious and

well marked as this difference is, it is nevertheless not essential, for it depends not on difference of quality between the fundamental chemical processes of plant and animal protoplasm, but merely on difference of rate or intensity. Both in the plant and in the animal, work springs out of the chemical transformation of material, but in the plant the process is relatively so slow that it must necessarily store up energy, not in the form of chemical compounds capable of producing work by their disintegration, but in the mechanical tension of elastic membranes. The plant-cell uses its material *continually* in tightening springs which it has the power of letting off at any required moment by virtue of that wonderful property of excitability which we have been studying this evening. Animal contractile protoplasm, and particularly that of muscle, does work only when required, and in doing so, uses its material directly. That this difference, great as it is, is not essential, we may learn further from the consideration that in those slow motions of the growing parts of plants which form the subject of Mr. Darwin's book, 'On the Movements of Plants,' there is no such storage of energy in tension of elastic membrane, there being plenty of time for the immediate transformation of chemical into mechanical work.

I have now concluded all that I have to say about the way in which plants and animals respond to external influences. In this evening's lecture you have seen exemplified the general fact, applicable alike to the physiology of plant and animal, that whatever knowledge we possess has been gained by experiment. In speaking of Mimosa, I might have entertained you with the ingenious conjectures which were formed as to its mechanism at a time when it was thought that we could arrive at knowledge by reasoning backwards—that is, by inferring from the structure of a living mechanism what its function is likely to be. In certain branches of physiology something has been learnt by this plan, but, as regards our present investigation, almost nothing, nor indeed could anything have been learnt. Everywhere we find that nature's means are adapted to her ends, and the more perfectly the better we know them. But, with rare exceptions, knowledge is got only by actually seeing her at work, for which purpose, if, as constantly happens, she uses concealment, we must tear off the veil, as you have seen this evening, by force. Have

we the right to assume this aggressive attitude? Ought we not rather to maintain one of reverent contemplation—waiting till the truth comes to us?

I will not attempt to answer this question, for no thoughtful person ever asked it in earnest. Another question lies behind it, which is a deeper and much older one. Is it worth while? Is the knowledge we seek worth having when we have got it? Notwithstanding that so recently even those who are least conversant with our work have been compelled to acknowledge the beauty and completeness of a life devoted to biological studies, still the question is pressed upon us every hour—how can you think of spending days in striving to unravel the mechanism of a leaf, when you know all the time that if there were no such thing as Dionaea, the world would not be less virtuous or less happy? This question, like the other, I willingly leave to those who put it. From their point of view it does not admit of an answer; from mine it does not require one. They must go on seeking for and finding virtue and happiness after their fashion; we must go on after ours, striving, by patient continuance in earnest work, to learn year by year some new truth of nature, or to understand some old one better. In so doing, we believe that we also have our reward.

[J. B. S.]

THE STUDY OF PHYSIOLOGY

ITS RELATION TO OTHER STUDIES AND ITS USE AS A PREPARATION FOR THAT OF MEDICINE[1]

THE custom of beginning a course of academical instruction by an introductory Lecture addressed to a general audience is, in the opinion of many judicious persons, one which, on ordinary occasions, may be advantageously dispensed with. The circumstances under which I now address you seem to be exceptional. I have come to Oxford as a stranger, not to teach a new subject, but to fill a new place. As every one here present probably knows, the accomplished biologist who, up to the time of his lamented death, held the office of Linacre Professor of Physiology here, included in the wide scope of his teaching the whole subject of Biology, bringing to the task an amount and variety of special knowledge, and a wealth of illustration derived from his familiarity with other subjects, which were the admiration of all who came into relation with him, either as a teacher or as a man of science.

It is not my business to-day to explain to you why it has been judged expedient to establish in Oxford a separate Professorship of Physiology. It will be sufficient if I state to you that in making the separation this University has followed the example of other great schools of Biology, and that the severance of the two branches of the subject is demanded, not only on account of their divergence from each other in respect of method and matter, but still more on account of the enormous extent of both subjects.

I shall endeavour to-day first to tell you what is the meaning of Physiology, and what is its relation to the studies which are carried on here in preparation for it, and then of its uses and practical application. With reference to this last, I must ask permission of my audience to address what I have to say specially to those who are looking forward to entering on the profession of Medicine.

Physiology belongs to a group of studies which have this

[1] A Public Lecture delivered at Oxford, in 1883.

in common, that they consist in the observation of the changes which go on in the universe, and in the determination of the order of these changes—in other words, in the investigation of the order of nature.

These studies are largely biological, for a chief element of change at the surface of our planet is plant and animal life; some of them relate to man exclusively, particularly those interesting inquiries which have in later times been brought together under the comprehensive word anthropology, and which serve as a link between biology and archaeology; but we usually restrict the term biological to those which regard man, not as man, but as an animal, i.e. in respect of all those endowments which he has in common with what he is pleased to call the inferior creatures. These studies are naturally divided, as well in respect of method as of subject, into two, namely, morphology and physiology. The former taking its departure from the very elementary observation that all living things grow, and that in this respect (that of growth) man stands on the same ground as plants and animals, inquires how animals and plants have become what they are. It as naturally divides itself into two branches, namely, the determination of the becoming of the individual, embryology—the question how the individual organism, whether plant, animal, or man, has unfolded itself from a germ of extreme simplicity—and phylogeny, the question how the tribe of plants or animals has undergone a corresponding evolution in the long process of time.

Physiology accepts and uses all that the morphologist has ascertained as to the form of organisms and as to the way in which they have been built up, but takes as a further point of departure a fact almost as familiar as that of growth itself; that in addition to growth there are other and most manifold changes continually going on in the living body, with the outward manifestations of which every one is conversant; changes which are as unceasing as those of the ocean or of the atmosphere, but unlike them in this respect, that they are orderly, and, if not constant, rhythmical.

It is the sum or complex of these changes, whether visible or invisible, external or internal, which constitutes what the physiologist means when he uses the word life; but he does not for a moment forget that just as the biography of each individual forms part, however insig-

nificant a part it may be, of the history of the nation, so the chemical and physical processes which are at this moment going on in every part of the living body of every human being, animal, or plant, are referable to the same *order* which expresses itself in the morphological development of the whole. As terms for this order, the morphologist and the physiologist alike use such words as adaptedness, or the more widely-significant German word *Zweckmässigkeit*, both of which are the expressions of a conviction which seems naturally to arise in the observer's mind, he knows not how, in the contemplation of the processes of nature, whether these be physiological or morphological.

Since Darwin, whatever obscurity of meaning formerly beset such words as I have mentioned has disappeared, and it has been found that the method of regarding nature, which it was his life-work to exemplify in its application to the external forms and relations of plants and animals, is as useful in physiological studies as in those of the morphologist. In stating that the internal motions or processes of animal and plant life may be regarded as answers or reactions of the organism to antecedent or concomitant external conditions, we are indeed only giving fuller expression to a thought which at all times has been present to the mind of the physiologist as the immediate and spontaneous outcome of the effort to understand what he observes. This fundamental notion is the starting-point for the two great divisions of physiological work. These are (1) the determination of the manifold and ever-changing actual relations between the organism and all that is outside of it; and (2) the investigation of the nature of the nexus between influence and reaction. In this last, notwithstanding the enormous amount of work which has been done towards it during the past thirty years, we are as yet only beginners, for the end in view is nothing less than the acquirement of so complete an understanding of the workings of the living organism, that the external conditions being known under which an organism is placed it is possible to forecast its reaction. It is because this goal is so far distant, and perhaps unattainable, that physiologists usually regard their subject rather as an application of scientific methods to a well-defined field of investigation than as a science of itself. Our business is both to set ourselves problems and to solve them. In the setting of them we have help from the morphologist, for it

is the structure of organs which suggests the question of their function or purpose ; but for the means of solving them we have recourse to the chemist and physicist, using the means we borrow from them, just as they give them to us.

I shall, I think, best illustrate what I have said by bringing you to the frontier lines at which Physiology borders with other regions of thought and work, and for this purpose must inquire a little more minutely than we have done as yet into the nature of the questions with which the physiologist concerns himself.

Let us assume it to be understood that Physiology is the knowledge of the PROCESSES which normally have their seat in the living substance, structure, or framework of the living plant, animal, or man. These we find to be of two kinds, namely, those which are present themselves in non-living matter, when under the same conditions, and those which go on under conditions peculiar to the organism, and which we know only by their physical effects, namely, by the physical changes which accompany them.

The phenomena of the former class we naturally divide for the purposes of study and investigation into chemical, mechanical, thermal, electrical, and so forth, these words implying fundamental distinctions, not so much in the nature of the phenomena, as in the way in which they must be observed and studied.

The processes of the second class are, for the reason already mentioned, often spoken of as vital, first, because they are peculiar to living organisms ; secondly, because they manifest themselves chiefly, but not by any means exclusively, in the structures to which we attribute the highest functions—brain, nerves, and muscles ; and thirdly, because although we can investigate and measure the chemical, mechanical, thermal, and electrical changes which are associated with them, we are compelled to admit that there is an underlying process which we cannot bring our measuring instruments to bear upon. It was the illustrious A. v. Haller who first gave definite expression to the distinction which I have been indicating. The muscular parts—those which, when touched or irritated, shrink (but in reality change their form only, not their volume), he called irritable.[1] The nerves by which, in man and

[1] 'De partibus corporis humani sentientibus et irritabilibus.' Op. minora, tom. I. xiv. It is to be noted, that although Haller under-

animals, the brain receives impressions from the external world, and influences and regulates the action of the limbs and of the internal muscular and secreting organs, he saw to be endowed with another property analogous to it, but differing from it in its mode of manifestation, and called that property sensibility. For a nerve, although it does not, as a muscle does, undergo any visible change when excited or irritated, evidently does undergo an invisible change, because it produces shrinking of the muscles with which it is connected, either directly or through the sensorium. This property, by which a nerve, without undergoing visible change itself, responds to irritation by determining visible change in a muscular mechanism at a distance, Haller called sensibility, and thought to explain it on the crude hypothesis that the structure of a nerve is that of a tube containing liquid, capable of serving as a medium for the propagation of vibratory motion.

Haller's observations on irritability and sensibility exercised an extraordinary influence, for the doctrine which he founded on them was perverted by contemporary physicians into a theory of disease, which became the basis of a system of medical practice.[1] In this way both words have acquired a meaning different from that which Haller assigned to them, so that it has become necessary to discontinue their use. In reality the two properties which he designated by them are much more closely related than he supposed, there being in both a common element which we now ordinarily express by saying that all the structures or organs which Haller regarded as 'sensible' or 'irritable' are *excitable*. By this is meant, first, that every such structure undergoes a molecular change at the point which is touched or excited, whether this is done by light, as in the case of the retina, by sound, as in that of the internal ear, or by any other kind of motion; and secondly, that this molecular change is propagated in definite directions

stood irritability to be the property of any part which shortens when touched ('quae ab externo aliquo contactu brevior fit'), he regarded sensibility as a quality of the whole organism. He made no distinction in this respect between nerve and centre.

[1] The Brunonian system. Its author, Dr. John Brown, published his best-known book in 1780. Its fundamental doctrine is that irritability is a property of the organism as a whole, and that it is maintained by a moderate amount of stimulation. Diseases are supposed to be dependent on over-stimulation (sthenic diseases) or insufficient stimulation (asthenic). Any external cause or condition which influences the body is termed a stimulus.

in the living organism, giving rise to similar molecular changes in other parts.

Now the systems of organs in which the phenomena of excitability chiefly, but not by any means exclusively, manifest themselves in the animal body, namely, the brain, spinal cord, muscles, and the organs of common and special sense, constitute the apparatus by which the animal maintains its relation with the external world. And if we confine our attention to animals low in the scale of intelligence, we find that the whole conduct of the individual, everything that we can observe as to its motives and motions, can be satisfactorily accounted for on two assumptions, namely, first, that of the existence in its nervous system, i.e. its brain, organs of sense, nerves and muscles of a property of excitability such as I have described and defined; and secondly, that there exist in the same organs channels of great, but by no means inconceivably great, complexity, along which waves of excitation must pass. This inference, the value and extent of meaning of which depend on the completeness with which we are able to judge by observations of the behaviour of animals low in the scale of intelligence, goes by the name of animal automatism.

I refer to it merely for the purpose of bringing you up to one of the lines of limitation of our subject of which I spoke just now. The observations on which it is founded have been chiefly made on the frog, among the most important and interesting being those contained in the classical essay of Professor Goltz.[1] When we extend them to creatures of high intelligence, such as the dog, the difficulties are greater, but still not such as might not be referred to the greater complexity of its intellectual life. However this may be, we come sooner or later into contact with a question which the methods which physiology employs cannot solve. The physiological student learns that in man the same mechanism exists as in the higher mammalia, and that from them we may descend by insensible gradations to the relatively unintelligent frog. If he realizes this fact he is inevitably brought face to face with the question, how far man also is an automaton. With reference to this question, I would observe that although as it stands it is one which cannot be investigated by our

[1] *Beiträge zur Lehre von den Functionem der Nervencentren des Frosches.* (Berlin, 1869.)

methods, it suggests an inquiry which is not only possible but necessary and unavoidable; that, namely, which relates to what is often called the mechanics of the central nervous system. The effort to explain even the most complicated acts as manifestations of the reaction of a living machinery to influences from outside, involving as it does an intimate knowledge of the structure and functions of that living mechanism and of all its parts, is one to which some of our best workers are giving their best work; but none of these men commits the gross error of supposing that the psychical concomitants of the excitatory process in the brain are things which our methods can help to explain.

Towards the problem of the nature of these concomitants we can contribute nothing, simply because they are not things which we can compare with any standards we possess. All that we can do about them is to localize them, but in accomplishing this, we are well aware that our researches neither help nor hinder us in the endeavour to penetrate the mystery of our own existence. All this is so plain that it would appear superfluous to state it, were there not persons who need to be informed on the subject; persons who imagine that, because our method is founded on the assumption that every material process is the product of material influences, every measurable effect the product of measurable causes, we extend that method to things beyond our province, namely to things which cannot be measured. A physiologist may be a philosopher if he has the gift for it, but from the moment that he enters the field of philosophy he leaves his tools behind him.

In observing the action of an organism, irrespectively of its form and development, we have before us, as we have seen, two distinct subjects of inquiry, namely, first, the nature of the chemical and physical processes which constitute its life, and secondly, the way in which by virtue of the property of which I have spoken, that of excitability, these varied and complicated changes become mere answers to external impressions, and are thus regulated for the good of the whole. And we have further seen that in pursuing the inquiry from the simpler organisms to the more complicated, we are checked, not by the complexity of the phenomena, but by the encounter with something else which, as physiologists, we have no means of grasping.

Let us now proceed in the opposite direction, namely, downwards from the organism to the material from which it must have originated, and we shall come to a limit which is quite as sharply defined, but not of the same nature, with the one we have left. Let us suppose that we have before us a solution of a salt of ammonium, which is as limpid as water, and is known to be chemically pure ; i.e. it contains nothing but the salt dissolved in it, and the gases of the atmosphere. After a time it is found that this liquid, which is now absolutely homogeneous, has ceased to be so —that it is peopled with little masses of apparently homogeneous material but definite form, each of which grows as a tree grows, and reacts to external influences as an animal does. This experiment, in which material just before apparently lifeless, becomes, as it were of itself, replete with life, has of late years become familiar to every one, and has attracted to itself a special interest, because it is supposed to have to do with the origin of living from non-living matter. In reality it has little to do with this question, for it is easy to show that if common air and common water are excluded, the experiment fails, i.e. no living organisms come into existence. Notwithstanding, however, that this particular case has nothing to do with it, the question exists and is still unanswered. How, then, does it happen that year after year it remains untouched—put on the shelf as it were? Not because we are afraid of the issue, or because we regard it as outside of our province, like the question of the relation of consciousness to material processes, but simply because there appears at present to be no probability that the conditions for an experimental testing of the question will be realized. The moment that an instance of abiogenesis, as it has been called, presents itself, the duty of investigating it is at once recognized, and many investigations have been undertaken by able men for this purpose. Although these have yielded fruits of importance and value in relation to the physical and chemical conditions under which the simplest forms of life exist, and to their relation to higher organisms, they have been, as regards the main question, barren and unfruitful.

And now I come to the second part of my subject, namely, to that of the use of physiological studies. And I will take it for granted that a chief purpose which we have in view in teaching Physiology in Oxford, is that we may prepare men who are specially fitted for the exercise of the Pro-

fession of Medicine—the art of healing. Is the knowledge of physiology necessary as a preparation for medicine? If it is, what sort of knowledge?

In attempting to answer the first question we must not content ourselves with such vague statements as that, inasmuch as medicine concerns the human body and its processes, physiology, which relates to the same things, must be useful to the physician. All this is true, but the argument has this weak point in it—that it does not sufficiently take into account that medicine is not a science but an art. In other words, if we want to determine rightly whether the future doctor is likely to be the better for what he learns in this Museum, we must take as our standard of fitness, practical duty.

Let us try, then, to realize what it is to be at the bedside of a patient who is dangerously ill, and to have to ask oneself the question whether all has been done for him that can be done; or to be concerned in a surgical operation, in which the result depends as much on conscientious and skilful after-care, in warding off the risks of infection, as in dexterous handling of the knife; or, finally, to be face to face with an outbreak of cholera or typhoid fever, and to have on the spot to devise and carry out the best means of prevention. Are our studies here calculated to fit men for these and like responsibilities and duties or not? The question is one which demands the gravest consideration, and all the more that, in my own profession, there are many who are inclined to believe that any such preparation is useless.

In relation to conduct and responsibility, the position of the doctor is in this respect simpler than that of the statesman or of the lawyer, that he has directly to do with natural processes. Guided by the empirical rules of his art he interferes in a certain way with the living mechanism, so as to do either harm or good. In the eye of the law, and to his fellow men, he is justified, or the contrary, according as he can show that he has acted in conformity with the rules of practice, i.e. with the prevailing opinion at the time as to what is right. But his own conscience demands something much higher than this. Every strong and true man recognizes it as his duty to act with a knowledge of the consequences of his action—to know what he is about in the fullest sense of the expression. To possess this knowledge, as I hope to be able to show you, he must be

a physiologist, for the things about which he is to know are such as come exclusively within the range of physiological work and study.

But I feel that in saying this I must carefully guard against misunderstanding. We must not allow this consideration to betray us into the mistake, of which we have several examples in the history of medicine during the last half of the eighteenth century and the beginning of the present, of supposing that a system of practice can be built up on any other foundation than that of experience. We have already seen that the only way to understand the problems of life is to apply to them the known laws of physics and chemistry, and that in so far physiology is only a part of natural philosophy; but any attempt to reverse the process, i.e. to deduce the relations of vital phenomena from those of non-living matter without direct physiological experiment, we know by experience to be futile. In like manner the physiologist who attempts to explain the origin and nature of disease on physiological principles, without subjecting these principles in their new applications to the test of experiment, commits an error of the same kind and incurs a similar risk; for what physiology gives to medicine is not principles, but methods of observation. The title of Institutes of Medicine, which is sometimes applied to it, really belongs to another branch of scientific inquiry, that which consists in the experimental investigation of the nature and origin of diseases, and of the external agents which either produce or counteract them. I must ask your attention for a few moments to this subject, because it is the link which connects the studies with which we occupy ourselves here, with the professional studies which will at a later period engage the attention of those of you who may intend to enter the medical profession.

The subject I have referred to is called Pathology. Let me endeavour to explain what the word means. Forty years ago the teaching of Pathology in the great medical schools of this country and Germany, consisted of a lengthy historical discussion of the successive systems of medical doctrine which up to that time followed each other at intervals of something like ten years.[1] Whatever influence

[1] The position of Pathology in 1843 (forty years ago) may be best judged of from Lotze's *Allgemeine Pathologie u. Therapie als mechanische Naturwissenschaften*, 1848, from Henle's *Handbuch der rationellen*

such teaching may have had on thoughtful minds at once disappeared in presence of the great discoveries in physics, in chemistry, and in biology, which marked the period I have mentioned. What we now mean by Pathology has come into existence quite independently of the other, and has no relation to it. It does not concern itself much with what is thought, and still less with what has been thought in former times about disease. It may be most truly described as the combined result of the observations and experiments of men of the last and present generation, among whose names some of the most distinguished are of men still in active life. It seeks for information as to the nature and causes of disease by two methods. One of these, the oldest and the best known, consists in the systematic study of diseases in those great institutions in which large numbers of sick persons are brought together for their own good, but also for that of the community. For our great hospitals are not merely charities, or even places of education, but at the same time pathological observatories, for the acquirement of knowledge valuable to all. The other method is the comparative one, its foundation being the study of disease in the lower animals, in their relation to the corresponding diseases in man. As this comparative method is more recent, and therefore less generally known, I will ask permission to say a word or two as to its nature.

Just as in physiology we derive nine-tenths, or an even larger proportion, of the knowledge we possess from the observation of animals, so in pathology we gain a similar advantage from the investigation of animal diseases. That this source of information, until recently, has not been sufficiently appreciated is very easily understood. In the first place the community of nature between animals and man was not realized. It was forgotten that a dog is 'fed with the same food, hurt by the same weapons, subject to the same diseases as' we are. Another reason is, that the close relation which ought to exist between the medical art and that which undertakes the treatment of animals, has been hitherto severed by social and professional distinctions, so that neither profession could profit by the

Pathologie, 1846, and from Helmholtz' *Das Denken in der Medicin*, 1878. It is remarkable that these three men, who were all at that time teaching pathology in different universities, have severally relinquished that subject for others. Helmholtz became first a physiologist, then a physicist; Lotze a psychologist, and Henle an anatomist.

other's knowledge and experience. I will try to illustrate the uses of the comparative method in Pathology by one or two instances.

Among the most fatal of the ills to which human nature is subject are the so-called traumatic diseases, i.e. those consequent on external injuries or wounds, which do not, as they ought, immediately heal. Still more fatal are the more slowly-developing tubercular diseases, which account for a very large proportion of the total mortality of this country. After these come the acute infective fevers, typhoid, scarlatina, &c., which, if they are dreaded more than the others, it is probably because we know more about them. If these three groups of maladies could be annihilated or brought under control, there would remain very little for much-suffering mortals to complain of. The first two are common to man and animals, and can be communicated from the one to the other. Of the third, most are peculiar to man, but a few are common. What practical use can we make of these facts?

During the last twenty years two discoveries in pathology have been made, both of them of great value in practical medicine, and both of them due to the comparative method. The first is the discovery that the traumatic diseases of which I have spoken arise, not from the trauma itself, but from the development of septic processes in it, and that this can be prevented by mechanical and chemical means; a discovery the value of which this University, two years ago, recognized by conferring its honorary degree on the great surgeon who made it. The second is that of the nature of the material cause—the contagium, as it is technically called—of the infective fevers.

Now both of these discoveries—the first indirectly (for Lister's earliest studies of traumatic disease related to the lower animals), the second directly and exclusively—were the offspring of the comparative method, the history of the latter being shortly as follows.

About twenty-eight years ago, the fact was discovered simultaneously in France and Germany, that a peculiar microscopical organism, with which every one is now familiar, existed in the blood of animals suffering from malignant anthrax, or, as it is called in France, *Charbon*; but, as so often happens, this discovery remained almost fruitless, until seven or eight years ago a practitioner holding a position analogous to that of medical officer of

health in a country town in Germany, the name of which you have probably never heard, set to work to investigate the subject, and furnished the experimental proof that the material cause of this particular disease is a plant capable of being cultivated outside of the body of the animal, which is, as the botanists say, its habitat.

If this discovery of Dr. Koch had been made only for the benefit of the thousands of cattle that die annually of Siberian Pest, as it is called, its interest for us would have been great, but much less than it actually is. Its chief value lies not in the immediate benefit conferred on cattle, but in its ulterior results for man. A few years before the time I have mentioned, it had been proved that tubercular disease is communicable from man to animals, and is therefore infective. In 1882, Dr. Koch[1], applying in a new direction the same modes of experimentation which had led him to his first discovery, found that the tubercular diseases which are so fatal to man, stand in the same relation to an external, and therefore in its nature exterminable, cause as anthrax, a kind of knowledge which will, beyond the possibility of doubt, serve as the foundation for an efficient prophylaxis against pulmonary consumption and the other less familiar forms of tubercular disease connected with it by community of origin and of issue.

I have given you this hasty sketch of the science of medicine or pathology, because I did not see how the question which now interests us could be properly answered without our having some understanding of its aims and nature.

In ordinary medical practice, a man is guided, as I have already said, by certain technical rules, which are or ought to be simply the embodiment of common experience as to the best way of dealing with particular cases. He requires, therefore, first, knowledge of these empirical rules (this being what is meant by technical knowledge), and secondly, skill in carrying them out. In ninety-nine cases out of a hundred these are sufficient guides, but in the hundredth his empirical knowledge fails him, and he has to fall back on knowledge which either he himself possesses or some other person, of the nature and tendency of the disease, or

[1] Dr. Koch published his first paper on Anthrax in Cohn's *Beiträge zur Biologie der Pflanzen*, Bd. ii. p. 286. The announcement of his discoveries as to the nature of the tuberculous infection was made in the *Berliner klinische Wochenschrift*, No. 15, 1882.

of the action of remedies, in other words, on his knowledge of pathology. Similarly in the exercise of the other branch of the medical art, that which aims at the prevention of disease (often miscalled sanitary science), the physician depends, or ought to depend, entirely on his knowledge of the causes and nature of diseases, i. e. once more on pathology; and this, I may venture to add, is the only subject which it is of any real importance that he should know.

Thus in the three kinds of duty which the doctor owes to the community, namely, first, to find out what is the matter with a sick man and foretell the issue of his disease; secondly, to do the best that can be done to counteract it; and thirdly, to guard against the causes of disease, the physician who takes the highest view of his responsibilities requires to be a pathologist. And if you are satisfied that pathology has been to-day rightly defined as the application, not of the principles of physiology, but of its methods to the study of the causes and nature of disease and of the mode of action of remedies, then you have good and sufficient reasons for the statement, that in order to know pathology you must have studied physiology, and to know physiology you must also have studied physics and chemistry. You have thus before you the evidence that the course of work you have to go through in preparation for the final examination in the Natural Science School, is not undertaken for the purpose of acquiring an honourable position, nor even as a training of your intellects for future effort, but as the best practical preparation for the divine art you hope in future to exercise.

And now I come to my final question. How ought physiology to be taught and studied in order that it may best accomplish its purpose as a *Vorbildung* for medicine? and I answer, where it most closely touches pathology. Virchow, who deserves more than any man of present or past times the title of Founder of Pathology, called the book in which he set forth the outlines of the subject by the title, Cellular Pathology.[1] The term was perhaps open to objection, but he had a meaning in using it which you get at at once if you substitute for cellular, elementary. The pathology which Virchow taught, and which has exercised so powerful an influence on medicine during the last thirty

[1] Virchow, *Die Cellularpathologie in ihrer Begründung auf physiologische u. pathologische Gewebelehre.* (Berlin, 1858.)

years—not, I once more repeat, by its doctrines but its method—was elementary pathology, the way in which diseases and their remedies affect the elementary processes of the organism, in other words, the functions of protoplasm.

In like manner it is elementary physiology chiefly which you will have to work at here. You come to us, knowing nothing of what is called anatomy—nothing of the structure either of plant or animal, excepting so much as may have come to you without study—or, perhaps, just enough to have inspired you with the desire of knowing a little more. You witness for the first time with a scientific purpose, i.e. with a purpose to understand the causes of things, the manifold motions and processes of the living organism, and you learn the great Darwinian truth that these all work together in the way I have tried to explain to you to-day for the good of the whole. At the same time you discover that, whether mechanical or chemical, visible or molecular, all this work is done by living elements of extreme simplicity, which in the living laboratory perform the functions both of workers and of apparatus, so that the knowledge of the endowments of these elementary structures constitutes much more than the half of what you have to learn.

Perhaps some of you are aware that in the great schools of medicine in this country as well as abroad, the order of preparatory study is different. It is thought desirable that the student should, before he attempts to understand the functions of the animal organism, learn the anatomy of man, and that, even after he has entered on his physiological studies, he should devote the greater part of his time to the studies of the dissecting-room. It is practically most advantageous to begin with anatomy, but there seems to be no reason why the anatomy first learnt should be that of man. However this may be, I think there can be no doubt that the position which has been for many years claimed for descriptive human anatomy in medical education is a wrong one.

At one time, say fifty years ago, the study of anatomy by dissection was the only practical work by which the student could train his powers of observation. There were no morphological or physiological laboratories, and scarcely any laboratories of chemistry in which students had the opportunity of learning the science practically. This is no longer the case, and consequently, as a mere discipline anatomy is scarcely wanted. There are, however, several

purposes for which it must be thoroughly mastered: first, because it is an indispensable technical method as well in the study of morphology as in that of physiology; and secondly, because it is required for the purposes of medicine and surgery.

In our great medical schools the ends of anatomical study are, I am afraid, sometimes lost sight of. Why should this subject, which in relation to morphology and physiology is so full of interest, be made as dry as the bones which the beginner handles? Why should he be compelled to devote as minute attention to details of structure of which the meaning is unknown, as to those of which the form or arrangement throws light on the development of the individual or of the race, or to those of which the functions are vital, or the anatomical relations are of interest in surgery or medicine? The only answer I ever heard to this question is that human anatomy is unrivalled as a subject to examine upon.

But let me not be misunderstood. The greater part of the practical work you will have to do with the Linacre Professor and myself will be anatomy, for our most important methods are anatomical. But it will not be dry anatomy; it will be anatomy studied with an intelligent purpose, with a view to something higher.

Be therefore of good courage. Enter on your new work with the assurance which I now confidently give you, that you will find it easy and interesting—easy as compared with what you have already mastered in natural science, and, provided that you possess that essential characteristic endowment of the naturalist—the desire to discover the causes of the things—more interesting than anything in which you have hitherto been engaged. But I must temper this encouragement with a warning. Our studies are what the Germans call *zeitraubend*. A great deal of time is required, and a great deal of devotion, to obtain a very moderate amount of knowledge—moderate, I mean, as compared with what you might know. The reason is that the field of study is limitless, and that, in order to understand what you are about, you must observe as well as read. Observing occupies, whether you will or not, a great deal of time. If either from circumstances or inclination you feel that you cannot give these, that is, I think, a sufficient reason for at once abandoning biology in favour of some other more congenial study.

ELEMENTARY PROBLEMS IN PHYSIOLOGY[1]

It has long ceased to be possible in the course of an annual address in Section D to give an account even of the most important advances which have been made during the preceding twelve months, in the various branches of knowledge which are now included under the term Biology. One reason is that each of the biological subjects has acquired such vast dimensions. The other, that the two main branches—Morphology, which strives to explain why plants and animals have assumed the forms and structure which they possess, and Physiology, which seeks to understand how the living organism works—have now diverged from each other so widely as regards subject and method, that there seems to be danger of complete separation of the one from the other.

From this sundering of sciences which a generation ago were intimately united, however inevitable it may be, Physiology chiefly suffers, as being even to the naturalist less attractive and interesting. The study of form and structure has the great advantage that it brings the observer into direct relation with objects which excite his curiosity without requiring too great an effort to understand them. This was the case even when Anatomy was mainly descriptive, and Zoology and Botany occupied themselves chiefly with classification and with definition of species. How much more is it the case now that Anatomy, Zoology, and Botany, have become built into one system of which the Doctrine of Evolution is the corner stone. Morphology, the name now given to this system, has, if I am not mistaken, this advantage over all other subjects of scientific study—that while attractive to the beginner, it is perfectly satisfactory to the mature student. It derives its perfectness from its subject—the *order* of the plant and animal world. For inasmuch as its fundamental conception is the development of all organisms, however complicated, from elementary

[1] Address to the Biological Section of the British Association, Newcastle-upon-Tyne, 1889.

forms, and as the theoretical development of the plant and animal world (in other words the science of morphology), claims to be nothing more than a synthesis of the observed facts of its actual development, the science is co-ordinate and conterminous with living nature and strives after a perfection which is that of nature itself.

Physiology is without this source of attractiveness. Its first lessons present difficulties to the beginner which, unless he is contented (as indeed ordinary students are) to accept as true what he does not understand, are, to say the least, discouraging; while to the more mature student who has mastered more or less some part of the subject, it fails to present a system of knowledge of which all the parts are interdependent and can be referred to one fundamental principle, comparable to that of development or evolution.

It is easy to understand that this must be so if we consider the present position of the subject, and the nature of the work which the physiologist has to do. That work is of two kinds. He has first to determine what are the chemical and physical endowments of living matter in general, and of each of the varieties of living matter which constitute the animal and plant organism in particular. Then, these having been investigated, he has to determine how these processes are localized so as to constitute the special function of each structure, and the relation between structure and process in each case. The order I have indicated is the logical order, but in the actual progress of physiology this order has not been followed, i. e. there has not been a correlation of structure with previously investigated process, for in former days physiologists spoke of assimilation, secretion, contraction and the like, as functions of muscles, glands, or other parts, without recognizing their ignorance of their real nature. But now, no one who is awake to the tendencies of thought and work in physiology, can fail to have observed that the best minds are directed with more concentration than ever before to those questions which relate to the elementary endowments of living matter, and that if they are still held in the background it is rather because of the extreme difficulty of approaching them than from any want of appreciation of their importance.

It is to some of these questions that I am anxious to draw the attention of the Section to-day. I feel that I have set myself a difficult task, but think that, even should I succeed very partially, the attempt may be a useful one. And I am

encouraged by the consideration that the interest they possess is one which is common to plant and animal physiology, and that if we really understood them, they would furnish a key, not only to the phenomena of nutrition and growth, but even to those of reproduction and development, and by the belief that it is in the direction of elementary physiology, which means nothing more than the study of the endowments of living material, that the advance of the next twenty years will be made.

Nearly fifty years ago, J. R. Mayer's[1] treatise on the relation between organic motion and the exchange of material in living organisms was published in Germany. Although its value was more appreciated by physicists than by biologists, it was in its purpose, as well as in its subject-matter, physiological. In it Mayer showed for the first time that certain functions of the animal body, which up to that time had been considered most vital, are strictly within reach of measurement, i. e. referable to physical standards of quantity. He was even able to demonstrate that those quantitative relations between different kinds of energy which physicists were then only beginning to recognize, held good as regards the processes peculiar to the living organism.

Almost immediately after the appearance of this now celebrated work, a series of discoveries were made in physiology, which constituted the period we are now considering an epoch. Mayer himself had proved that muscles in doing work and producing heat do not do so at the expense of their own substance. But this fact could not be understood until Bernard showed that sugar is one of the most important constituents of the blood, and its storage and production a chief function of the liver. Helmholtz next succeeded in proving what Johannes Müller[2] had declared to be nearly impossible, namely, that the time occupied by the propagation of a motor impulse from the brain to a muscle could be measured, and showed it to be proportional to the distance traversed. Next, du Bois-Reymond investigated the electrical phenomena of living beings, and marshalled them under a physical theory which stood its ground against the severest criticism for more than a generation. And finally the hydrodynamic principles

[1] J. R. Mayer, *Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel.* Heilbronn, 1845.

[2] Müller's *Physiology.* Translation of second edition, p. 729.

relating to the circulation, set forth by Dr. Thomas Young in his Croonian Lecture forty years before, were demonstrated experimentally by Ludwig, at the very time when Helmholtz was giving definite form to the great natural philosopher's Theory of Colour Perceptions.

The effect of these discoveries was to produce a complete revolution in the ways of thinking and speaking about the phenomena of life. The error of the past had been to believe that, although the heart resembled a pump, although digestion could be imitated in the laboratory, and comparisons of vital with physical processes could be used for illustration, it was always wrong to identify them. But, inasmuch as it had been learned that sensation is propagated along a nerve just as sound is propagated through the air, only with something like a tenth of the velocity; that the relations between the work done, the heat produced, and the fuel used, can be investigated in the living body just as they are in the steam engine, it now came to be felt that in other similar cases, what had been before regarded as peculiarly vital might be understood on physical principles, and that for the future the word 'vital' as distinctive of physiological processes might be abandoned altogether. In looking back, we have no difficulty in seeing that the lines of investigation which were then initiated by such men as Helmholtz, Ludwig, Brücke, du Bois-Reymond, Donders, Bernard, are those along which, during the succeeding generation, the science of physiology advanced; nor can any one who is acquainted with the literature of that time doubt that these leaders of physiological thought knew that they were the beginners of a new epoch. But such an epoch cannot occur again. We have adopted once for all the right, i.e. the scientific method, and there is not the least possibility of our recurring to the wrong. We have no new departure, no change of front in prospect; but even times which are not epochal have their tendencies, and I venture to submit to you, that in physiology the tendency of the present time is characterized by the concentration of the best efforts of the best minds on what I have already referred to as elementary questions. The work of investigating the special functions of organs, which during the last two decades has yielded such splendid results, is still proceeding, and every year new ground is being broken and new and fruitful lines of experimental inquiry are being opened up; but the further the physio-

logist advances in this work of analysis and differentiation, the more frequently does he find his attention arrested by deeper questions relating to the essential endowments of living matter, of which even the most highly differentiated functions of the animal or plant organism are the outcome. In our science the order of progress has been hitherto and will continue to be the reverse of the order of nature. Nature begins with the elementary and ends with the complex (first the amoeba, then the man). Our mode of investigation has to begin at the end. And this not merely for the historical reason that the first stimulus to physiological inquiry was man's reasonable desire to know himself, but because differentiation actually involves simplification. For just as in manufactures it is the effect of division of labour that less is required of each workman, so in an organism which is made up of many organs, the function of each is simpler.

Physiology, therefore, first studies man and the higher animals and proceeds to the higher plants, then to invertebrates and cryptogams, ending where development begins. From the beginning her aim has been to correlate function with structure, at first roughly, afterwards, when, as I have explained, her methods of observation became scientific, more and more accurately; the principle being that *every appreciable difference of structure corresponds to a difference of function*; and conversely that each endowment of a living organ must be explained, if explained at all, as springing from its structure.

It is not difficult to see whither this method must eventually lead us. For inasmuch as function is more complicated than structure, the result of proceeding, as physiology normally does, from structure to function, must inevitably be to bring us face to face with functional differences which have no structural difference to explain them. Thus, for example, if the physiologist undertakes to explain the function of a highly differentiated organ like the eye, he finds that up to a certain point, provided that he has the requisite knowledge of dioptrics, the method of correlation guides him straight to his point. He can mentally or actually construct an eye which will perform the functions of the real eye, in so far as the formation of a real image of the field of vision on the retina is concerned, and will be able thereby to understand how the retinal picture is transferred to the organ of consciousness. Having

arrived at this point he begins to correlate the known structure of the retina with what is required of it, and finds that the number of objects which he can discriminate in the field of vision is as numerous as, but not more numerous than, the parts of the retina, i.e. the cones which are concerned in discriminating them. So far he has no difficulty; but the method of correlation fails him from the moment that he considers that each object point in the field of vision is coloured, and that he is able to discriminate not merely the number and the relations of all the object points to each other, but the colour of each separately. He then sees at once that each cone must possess a plurality of endowments for which its structure affords no explanation. In other words, in the minute structure of the human retina, we have a mechanism which would completely explain the picture of which we are conscious, were the objects composing it colourless, i.e. possessed of one objective quality only, but it leaves us without explanation of the differentiation of colour.

Similarly, if we are called upon to explain the function of a secreting gland, such e.g. as the liver, there is no difficulty in understanding that, inasmuch as the whole gland consists of lobules which resemble each other exactly, and each lobule is similarly made up of cells which are all alike, each individual cell must be capable of performing all the functions of the whole organ. But when by exact experiment we learn that the liver possesses not one function but many—when we know that it is a storehouse for animal starch, that each cell possesses the power of separating waste colouring matter from the blood, and of manufacturing several kinds of crystallizable products, some of which it sends in one direction and others in the opposite, we find again that the correlation method fails us, and that all that our knowledge of the minute structure has done for us is to set before us a question which, though elementary, we are quite unable to answer.

By multiplying examples of the same kind, we should in each case come to the same issue, namely, *plurality of function with unity of structure*, the unity being represented by a simple structural element—be it retinal cone or cell—possessed of numerous endowments. Whenever this point is arrived at in any investigation, structure must for the moment cease to be our guide, and in general, two courses or alternatives are open to us. One is to fall back

on that worn-out *Deus ex machina*, protoplasm, as if it afforded a sufficient explanation of everything which cannot be explained otherwise, and accordingly to defer the consideration of the functions which have no demonstrable connexion with structure as for the present beyond the scope of investigation; the other is, retaining our hold of the fundamental principle of correlation, to take the problem in reverse, i. e. to use analysis of function as a guide to the ultra-microscopical analysis of structure.

I need scarcely say that of these two courses the *first* is wrong, the *second* right, for in following it we still hold to the fundamental principle that *living material acts by virtue of its structure*, provided that we allow the term structure to be used in a sense which carries it beyond the limits of anatomical investigation, i. e. beyond the knowledge which can be attained either by the scalpel or the microscope. We thus (as I have said) proceed from function to structure, instead of the other way.

The departure from the traditions of our science, which this change of direction seems to imply, is indeed more apparent than real. In tracing the history of some of the greatest advances, we find that the recognition of function has preceded the knowledge of structure. Haller's discovery of irritability was known and bore fruit, long before anything was known of the structure of muscle. So also, at a later period, Bichat was led by his recognition of the physiological differences between what he termed the functions of organic and animal life, to those anatomical researches which were the basis of the modern science of Histology. Again, in much more recent times, the investigation of the function of gland cells, which has been carried on with such remarkable results by Professor Heidenhain in Germany, and with equal success by Mr. Langley in this country, has led to the discovery of the structural changes which they undergo in passing from the state of repose to that of activity; nor could I mention a better example than that afforded (among many others relating to the physiology of the nervous system) by Dr. Gaskell's recent and very important discovery of the anatomical difference between cerebro-spinal nerves of different functions. We may therefore anticipate that the future of physiology will differ from the past chiefly in this respect —that whereas hitherto the greater part of the work has consisted in the interpretation of facts arrived at in the

first instance by anatomical methods of research, Histology, once the guide of Physiology, has now become her handmaid.

During the last ten or fifteen years histology has carried her methods of research to such a degree of perfection that further improvement scarcely seems possible. As compared with these subtle refinements, the 'minute anatomy' of thirty years ago appears coarse—the skill for which we once took credit seems but clumsiness. Notwithstanding, the problems of the future from their very nature lie as completely out of reach of the one as of the other. It is by different methods of investigation that our better equipped successors must gain insight of those vital processes of which even the ultimate results of microscopical analysis will ever be, as they are now, only the outward and visible signs.

The Invisible Mechanism of Life.

In what has preceded I have endeavoured to show that at present the fundamental questions in physiology, the problems which most urgently demand solution, are those which relate to the endowments of apparently structureless living matter, and that the most important part of the work of the immediate future will be the analysis of these endowments. With this view what we have to do is, first, to select those cases in which the vital process offers itself in its simplest form, and is consequently best understood; and, secondly, to inquire how far in these particular instances we may, taking as our guide the principle I have so often mentioned as fundamental, viz. the correlation of structure with function, of mechanism with action, proceed in drawing inferences as to the mechanism by which these vital processes are in these simplest cases actually carried out.

The most distinctive peculiarity of living matter as compared with non-living is that it is ever changing while ever the same, i.e. that life is a state of ceaseless change. For our present purpose I must ask you, first, to distinguish between two kinds of change which are equally characteristic of living organisms, namely, those of growth and decay on the one hand, and those of nutrition on the other. Growth the biologist calls evolution. Growth means the unfolding, i.e. development of the latent potentialities of form and structure which exist in the germ, and which it

has derived by inheritance. A growing organism is not the same to-day as it was yesterday, and consequently not quite the same now as it was a minute ago, and never again will be. This kind of change I am going to ask you to exclude from consideration altogether at this moment, for in truth it does not belong to Physiology but rather to Morphology, and to limit your attention to the other kind which includes all other vital phenomena. I designated it just now as nutrition, but this word expresses my meaning very inadequately. The term which has been used for half a century to designate the sum or complex of the non-developmental activities of an organism is 'exchange of material', for which Professor Foster has given the very acceptable substitute Metabolism. Metabolism is only another word for 'change', but in using it we understand it to mean that, although an organism in respect of its development may never be what it has been, the phases of alternate activity and repose which mark the flow of its life-stream are recurrent. Life is a Cyclosis in which the organism returns after every cycle to the same point of departure, ever changing yet ever the same.

It is this antithesis which constitutes the essential distinction between the two great branches of biology, the two opposite aspects in which the world of life presents itself to the inquiring mind of man. Seen from the morphological side the whole plant and animal kingdom constitutes the unfolding of a structural plan which was once latent in a form of living material of great apparent simplicity. From the physiological side this apparently simple material is seen to be capable of the discharge of functions of great complexity, and therefore must possess corresponding complexity of mechanism. It is the nature of this invisible mechanism that physiology thirsts to know. Although little progress has as yet been made, and little may as yet be possible, in satisfying this desire, yet, as I shall endeavour to show you, the existing knowledge of the subject has so far taken consistent form in the minds of the leaders of physiological thought, that it is now possible to distinguish the direction in which the soberest speculation is tending.

The *non-developmental* vital functions of protoplasm are the absorption of oxygen, the discharge of carbon dioxide and water and ammonia, the doing of mechanical work, the production of heat, light, and electricity. All these, excepting the last, are known to have chemical actions as their

inseparable concomitants. As regards electricity we have no proof of the dependence of the electrical properties of plants and animals on chemical action. But all the other activities which have been mentioned are fundamentally chemical.

The Ultra-Microscopical Structure of Living Material.

Let us first consider the relation of oxygen to living matter and vital process. For three-quarters of a century after the fundamental discoveries of Lavoisier and Priestley (1772–6), the accepted doctrine was that the effete matter of the body was brought to the lungs by the circulation and burnt there, of which fact the carbon dioxide expired seemed an obvious proof. Then came the discovery that arterial blood contained more oxygen than venous blood, and consequently that oxygen must be conveyed as such by the blood stream to do its purifying work in all parts of the body, this advance in the understanding of the process being crowned a few years later by the discovery of the oxygen-carrying properties of the colouring matter of the blood, in which the present President of the Royal Society took so prominent a part. Finally, between 1872 and 1876, as the result of an elaborate series of investigations of the respiratory process, the proof was given by Pflüger[1] that the function of oxygen in the living organism is not to destroy effete matter either here or there, but rather to serve as a food for protoplasm, which so long as it lives is capable of charging itself with this gas, absorbing it with such avidity, that although its own substance retains its integrity, no free oxygen can exist in its neighbourhood. This discovery, of which the importance is comparable with that of Lavoisier, can best be judged of by considering its influence on other fundamental conceptions of the vital process. The generally accepted notion of effete matter waiting to be oxidized was associated with a more general one, viz. that the elaborate structure of the body was not permanent, but constantly undergoing decay and renewal. What we have now learnt is, that the material to be oxidized comes as much from the outside as the oxygen which burns it, though the reaction between them, i.e. the

[1] Pflüger's *Archiv*, vol. vi, 1872, p, 43, and vol. x, 1875, p. 251. 'Ueber die physiologische Verbrennung in den lebendigen Organismen.'

oxidation, is intrinsic, i.e. takes place within the living molecular framework.

Protoplasm, therefore, understanding by the term the visible and tangible presentation to our senses of living material, comes to consist of two things—namely, of framework and of content—of channel and of stream—of acting part which lives and is stable—of acted-on part which has never lived and is labile, that is, in a state of metabolism, or chemical transformation.

If such be the relation between the living framework and the stream which bathes it, we must attribute to this living, stable, acting part a property which is characteristic of the bodies called in physiological language ferments, or enzymes, the property which, following Berzelius, we have for the last half-century expressed by the word *catalytic*; and use, without thereby claiming to understand it, to indicate a mode of action in which the agent which produces the change does not itself take part in the decompositions which it produces.

Micellae and Tagmata (Nägeli, Pfeffer).

I have brought you to this point as the outcome of what we know as to the essential nature of the all-important relation between oxygen and life. In botanical physiology the general notion of a stable catalysing framework, and of an interstitial labile material, which might be called catalyte, has been arrived at on quite other grounds. This notion is represented in plant physiology by two words, both of which correspond in meaning—Micellae, the word devised by Nägeli, and the better word, Tagmata, substituted for it by Pfeffer. Nägeli's word has been adopted by Professor Sachs as the expression of his own thought in relation to the ultra-microscopical structure of the protoplasm of the plant cell. His view is that certain well-known properties of organized bodies require for their explanation the admission that the simplest *visible* structure is itself made up of an arrangement of units of a far inferior order of minuteness. It is these hypothetical units that Nägeli has called Micellae.

Now, Nägeli [1] in the first instance confounded the micellae with molecules, conceiving that the molecule of living

[1] Nägeli, *Theorie der Gährung, Beitrag zur Molecular-Physiologie*, 1879, p. 121.

matter must be of enormous size. But, inasmuch as we have no reason for believing that any form of living material is chemically homogeneous, it was soon recognized, perhaps first by Pfeffer[1], but eventually also by Nägeli himself, that a micella, the ultimate element of living material, is not equivalent to a molecule, however big or complex, but must rather be an arrangement or phalanx of molecules of different kinds. Hence the word Tagma, first used by Pfeffer, has come to be accepted as best expressing the notion. And here it must be noted that each of the physiologists to whom reference has been made regards the micellae, not as a mere aggregate of separate particles, but as connected together so as to form a system —a conception which is in harmony with the view I gave you just now from the side of animal physiology, of catalysing framework and interstitial catalysable material.

To Professor Sachs, this porous constitution of protoplasm serves to explain the property of vital turgescence, that is, its power of charging itself with aqueous liquid—a power which Sachs estimates to be so enormous that living protoplasm may, he believes, be able to condense water which it takes into its interstices, to less than its normal volume. For our present purpose it is sufficient for us to understand that to the greatest botanical thinkers, as well as to the greatest animal physiologists, the ultimate mechanism by which life is carried on is not, as Professor Sachs[2] puts it, 'slime', but 'a very distensible and exceedingly fine network'.

Ino-tagmata (Engelmann, Pflüger, Bernstein).

And now let us try to get a step further by crossing back in thought from plants to animals. At first sight, the elementary vital processes of life seem more complicated in the animal than in the plant, but they are, on the contrary, simpler; for plant protoplasm, though it may be structurally homogeneous, is dynamically polyergic—it has many endowments—whereas in the animal organism there are cases in which a structure has only one function assigned to it. Of this the best examples are to be found among so-called excitable tissues, viz. those which are differentiated

[1] Pfeffer, *Pflanzenphysiologie*,, Leipsic, 1881, p. 12.
[2] Sachs, *Experimental-Physiologie*, 1865, p. 443; and *Lectures on the Physiology of Plants*, English translation, p. 206.

for the purpose of producing (along with heat) mechanical work, light, or electricity. In the life of the plant these endowments, if enjoyed at all, are enjoyed in common with others.

By the study, therefore, of muscle, of light organ and of electrical organ, the vital mechanism is more accessible than by any other portal. About light organs we as yet know little, but the little we know is of value. Of electrical organs rather more, about muscle a great deal.

To the case of muscle, Engelmann, one of the best observers and thinkers on the elementary questions which we have now before us, has transferred the terminology of Nägeli and Pfeffer as descriptive of the mechanism of its contraction. Muscular protoplasm differs from those kinds of living matter to which I have applied the term 'polyergic', in possessing a molecular structure comparable with that of a crystal in this respect, that each portion of the apparently homogeneous and transparent material of which it consists resembles every other.

With this ultra-microscopical structure, its structure as investigated by the microscope may be correlated, the central fact being that, just as a muscular fibre can be divided into cylinders by cross sections, so each such cylinder is made up of an indefinite number of inconceivably minute cylindrical parts, each of which is an epitome of the whole. These, Engelmann, following Pfeffer, calls ino-tagmata. So long as life lasts each minute phalanx has the power of keeping its axis parallel with those of its neighbours, and of so acting within its own sphere as to produce, whenever it is awakened from the state of rest to that of activity, a fluxion from poles to equator. In other words, muscle, like plant protoplasm, consists of a stable framework of living catalysing substance, which governs the mechanical and chemical changes which occur in the interstitial catalysable material, with this difference, that here the ultra-microscopical structure resembles that of a uniaxial crystal, whereas in plant protoplasm there may be no evidence of such arrangement.[1]

According to this scheme of muscular structure, the contraction, i.e. the change of form which, if allowed, a muscle undergoes when stimulated, has its seat not in the system of tagmata, but in the interstitial material which surrounds it, and consists in the migration of that labile material

[1] Brücke, *Vorlesungen*, 2nd edition, vol. ii, p. 497.

from pole to equator, this being synchronous with explosive oxidation, sudden disengagement of heat, and change in the electrical state of the living substance. Let us now see how far the scheme will help us to an understanding of this marvellous concomitance of chemical, electrical, and mechanical change.

It is not necessary to prove to you that the discharge of carbon dioxide and the production of heat which we know to be associated with that awakening of a muscle to activity which we call stimulation, are indices of oxidation. If we take this fact in connexion with the view that has just been given of the mechanism of contraction, it is obvious that there must be in the sphere of each tagma an accumulation of oxygen and oxidizable material, and that concomitantly with or antecedently to the migration of liquid from pole to equator, these must come into encounter. Let us for a moment suppose that a soluble carbohydrate is the catalysable material, that this is accumulated equatorially, and oxygen at the poles, and consequently that between equator and poles water and carbon dioxide, the only products of the explosion, are set free. That the process is really of this nature is the conclusion to which an elaborate study of the electrical phenomena which accompany it has led one of the most eminent physiologists of the present time, Professor Bernstein.[1] To this I wish for a moment to ask your attention.

Professor Bernstein's view of the molecular structure of muscular protoplasm is in entire accordance with the theory of Pflüger and with the scheme of Engelmann, with this addition, that each ino-tagma is electrically polarized when in a state of rest, depolarized at the moment of excitation or stimulation, and that the axes of the tagmata are so directed that they are always parallel to the surface of the fibre, and consequently have their positive sides exposed. In this amended form, the theory admits of being harmonized with the fundamental facts of muscle-electricity, namely, that cut surfaces are negative to sound surfaces, and excited parts to inactive, provided that the direction of the hypothetical polarization is from equator to pole, i. e. that in the resting state the poles of each tagma are charged with negative ions, the equators with positive ; and conse-

[1] Bernstein, 'Neue Theorie der Erregungsvorgänge und electrischen Erscheinungen an den Nerven- und Muskelfasern', *Untersuchungen aus dem Physiologischen Institut*, Halle, 1888.

quently that the direction of the discharge in the catalyte at the moment that the polarization disappears is from pole to equator.

Time forbids me even to attempt to explain how this theory enables us to express more consistently the accepted explanations of many collateral phenomena, particularly those of electrotonus. I am content to show you that it is not impossible to regard the three phenomena, viz. chemical explosion, sudden electrical change, and change of form, as all manifestations of one and the same process—as products of the same mechanism.

In plants, in certain organs or parts in which movement takes place as in muscles in response to stimulation, the physiological conditions are the same or similar, but the structural very different; for the effect is produced not by a change of form, but by a diminution of volume of the excited part, and this consists not of fibres but of cells. The way in which the diminution of volume of the whole organ is brought about is by diminution of the volume of each cell, an effect which can obviously be produced by flow of liquid out of the cell. At first sight therefore the differences are much more striking than the resemblances.

But it is not so in reality. For the more closely we fix our attention on the elementary process rather than on the external form, the stronger appears the analogy—the more complete the correspondence. The state of turgor, as it has been long called by botanical physiologists, by virtue of which the framework of the protoplasm of the plant retains its content with a tenacity to which I have already referred, is the analogue of the state of polarization of Bernstein. As regards its state of aggregation it can scarcely be doubted that inasmuch as the electrical concomitants of excitation of the plant cell so closely correspond with those of muscle, here also the tagmata are cylindrical, and have their axes parallel to each other. Beyond this we ought perhaps not to allow speculation to carry us, but it is scarcely possible to refrain from connecting this inference with the streaming motion of protoplasm which in living plant cells is one of the indices of vitality. If, as must I think be supposed, this movement is interstitial, i.e. due to the mechanical action of the moving protoplasm on itself, we can most readily understand its mechanism as consisting in rhythmically

recurring phases of close and open order in the direction of the tagmatic axes.

In submitting this hypothesis I do not for a moment forget that the facts relating to the contractility of plant cells have as yet been insufficiently investigated. No one has as yet shown that when the leaf of the sensitive plant falls, or that of the flytrap closes on its prey, heat is developed or oxidation takes place, but it does not seem to me very rash to anticipate that if it were possible to make the experiment to-morrow it would be found to be so.

Anabolism and Catabolism.

I have thus endeavoured, building on two principles in physiology, firstly, that of the constant correlation of mechanism and action, of structure and function, and secondly, the identity of plant and animal life both as regards mechanism and structure—and on two experimentally ascertained elementary relations, viz. the relation of living matter or protoplasm to water on the one hand and to oxygen and food on the other—to present to you in part the outline or sketch of what might, if I had time to complete it, be an adequate conception of the mechanism and process of life as it presents itself under the simplest conditions. To complete this outline, so far as I can to-day, I have but one other consideration to bring before you, one which is connected with the last of my four points of departure—that of the relation of oxygen to protoplasm, a relation which springs out of the avidity with which, without being oxidized or even sensibly altered in chemical constitution, it seizes upon oxygen and stores it for its own purposes. The consideration which this suggests is that if the oxygen and oxidizable material are constantly stored, they must either constantly or at intervals be discharged, and inasmuch as we know that in every instance without exception in which heat is produced or work is done, these processes have discharge of water and of carbon dioxide for their concomitants, we are justified in regarding these discharges as the sign of expenditure, the charging with oxygen as the sign of restitution. In other words, a new characteristic of living process springs out of those we have already had before us, namely, that it is a constantly recurring alternation of opposite and complementary states, that of activity or discharge, that of rest or restitution.

Is it so or is it not? In the minds of most physiologists the distinction between the phenomena of discharge and the phenomena of restitution (*Erholung*) is fundamental, but beyond this, unanimity ceases. Two distinguished men, one in Germany and one in England, I refer to Professor Hering and Dr. Gaskell, have taken, on independent grounds, a different view to the one above suggested, according to which life consists not of alternations between rest and activity, charge and discharge, loading and exploding, but between two kinds of activity, two kinds of explosion, which differ only in the direction in which they act, in the circumstance that they are antagonistic to each other.

Now when we compare the two processes of rest, which as regards living matter means restitution, and discharge which means action, with each other, they may further be distinguished in this respect, that, whereas restitution is autonomic, i.e. goes on continuously like the administrative functions of a well-ordered community, the other is occasional, i.e. takes place only at the suggestion of external influences; that, in other words, the contrast between action and rest is (in relation to protoplasm) essentially the same as between waking and sleeping.

It is in accordance with this analogy between the alternation of waking and sleeping of the whole organism, and the corresponding alternation of restitution and discharge, of every kind of living substance, that physiologists by common consent use the term Stimulus (*Reiz*), meaning thereby nothing more than that it is by external disturbing or interfering influence of some kind that energies stored in living material are (for the most part suddenly) discharged. Now, if I were to maintain that restitution is not autonomic, but determined, as waking is, by an external stimulus—that it differed from waking only in the direction in which the stimulation acts, i.e. in the tendency towards construction on the one hand, towards destruction on the other, I should fairly and as clearly as possible express the doctrine which, as I have said, the two distinguished teachers I have mentioned, viz. Dr. Gaskell and Professor Hering[1], have embodied in words which have now become familiar to every student. The words in

[1] Hering, *Zur Theorie der Vorgänge in der lebendigen Substanz*, Prague, 1888, pp. 1–22. See also a paper by Dr. Gaskell in Ludwig's *Festschrift*, 1888, Leipsic, p. 115.

question, 'anabolism', which being interpreted means winding up, and 'catabolism', running down, are the creation of Dr. Gaskell. Professor Hering's equivalents for these are 'assimilation', which, of course, means storage of oxygen and oxidizable material, and 'disassimilation', discharge of these in the altered form of carbon dioxide and water. But the point of the theory which attaches to them lies in this, that that wonderful power which living material enjoys of continually building itself up out of its environment, is, as I have already suggested, not autonomic, but just as dependent on occasional and external influences, or stimuli, as we know the disintegrating processes to be; and accordingly Hering finds it necessary to include under the term stimuli not only those which determine action, but to create a new class of stimuli which he calls *Assimilations-Reize*, those which, instead of waking living mechanism to action, provoke it to rest.

It is unfortunately impossible within the compass of an address like the present to place before you the wide range of experimental facts which have led two of the strongest intellects of our time to adopt a theory which, when looked at *a priori*, seems so contradictory. I must content myself with mentioning that Hering was led to it chiefly by the study of one of the examples to which I referred in my introduction, namely, the colour-discriminating function of the retina. Dr. Gaskell by the study of that very instructive class of phenomena which reveal to us that among the channels by which the brain maintains its sovereign power as supreme regulator of all the complicated processes which go on in the different parts of the animal organism, there are some which convey only commands to action, others commands to rest, the former being called by Gaskell catabolic, the latter anabolic. To go further than this would not only wear out your patience but would carry me beyond the limits I proposed to myself, viz. the mechanism of life in its simplest aspects. I therefore leave the subject here, adding one word only. The distinction which has suggested to their authors the words on which I have been commenting is a real one, but it implies rather the interference with each other of the simultaneous operation of two regulating mechanisms, than an antagonism between two processes of opposite tendencies carried on by the same mechanism; or, putting it otherwise, that the observed antagonism is

between one nervous mechanism and another, and not between two antagonistic functions of the same living material.

Vitalism.

Without attempting to recapitulate, I have a word to say by way of conclusion on a question which may probably have suggested itself to some of my audience.

I have indicated to you that although scientific thought does not, like speculative, oscillate from side to side, but marches forward with a continued and uninterrupted progress, the stages of that progress may be marked by characteristic tendencies; and I have endeavoured to show that in physiology the questions which concentrate to themselves the most lively interest are those which lie at the basis of the elementary mechanism of life.

The word Life is used in physiology in what, if you like, may be called a technical sense, and denotes only that state of *change with permanence* which I have endeavoured to set forth to you. In this restricted sense of the word, therefore, the question 'What is Life?' is one to which the answer is approachable; but I need not say that in a higher sense—higher because it appeals to higher faculties in our nature—the word suggests something outside of mechanism, which may perchance be its cause rather than its effect.

The tendency to recognize such a relation as this is what we mean by vitalism. At the beginning of this discourse I referred to the anti-vitalistic tendency which accompanied the great advance of knowledge that took place at the middle of the century. But even at the height of this movement there was a reaction towards vitalism, of which Virchow, the founder of modern pathology, was the greatest exponent. Now, a generation later, a tendency in the same direction is manifesting itself in various quarters. What does this tendency mean? It has to my mind the same significance now that it had then. Thirty years ago the discovery of the cell as the basis of vital function was new, and the mystery which before belonged to the organism was transferred to the unit, which while it served to explain everything was itself unexplained. The discovery

[1] Virchow, 'Alter und neuer Vitalismus', *Archiv für path. Anat.*, 1856, vol. ix, p. 1. See also Rindfleisch, *Ärztliche Philosophie*, Würzburg, 1888, pp. 10–13.

of the cell seemed to be a very close approach to the mechanism of life, but now we are striving to get even closer, and with the same result. Our measurements are more exact, our methods finer; but these very methods bring us to close quarters with phenomena which, although within reach of exact investigation, are as regards their essence involved in a mystery which is the more profound the more it is brought into contrast with the exact knowledge we possess of surrounding conditions.

If what I have said is true, there is little ground for the apprehension that exists in the minds of some that the habit of scrutinizing the mechanism of life tends to make men regard what can be so learned as the only kind of knowledge. The tendency is now certainly rather in the other direction. What we have to guard against is the mixing of two methods, and, so far as we are concerned, the intrusion into our subject of philosophical speculation. Let us willingly and with our hearts do homage to 'divine Philosophy', but let that homage be rendered outsidethe limits of our science. Let those who are so inclined cross the frontier and philosophize; but to me it appears to be more conducive to progress that we should do our best to furnish professed philosophers with such facts relating to structure and mechanism as may serve them as aids in the investigation of those deeper problems which concern man's relations to the past, the present, and the unknown future.

THE ORIGIN AND MEANING OF THE TERM 'BIOLOGY'[1]

We are assembled this evening as representatives of the sciences—men and women who seek to advance knowledge by scientific methods. The common ground on which we stand is that of belief in the paramount value of the end for which we are striving, of its inherent power to make men wiser, happier, and better; and our common purpose is to strengthen and encourage one another in our efforts for its attainment. We have come to learn what progress has been made in departments of knowledge which lie outside of our own special scientific interests and occupations, to widen our views, and to correct whatever misconceptions may have arisen from the necessity which limits each of us to his own field of study; and, above all, we are here for the purpose of bringing our divided energies into effectual and combined action.

Probably few of the members of the Association are fully aware of the influence which it has exercised during the last half-century and more in furthering the scientific development of this country. Wide as is the range of its activity, there has been no great question in the field of scientific inquiry which it has failed to discuss; no important line of investigation which it has not promoted; no great discovery which it has not welcomed. After more than sixty years of existence it still finds itself in the energy of middle life, looking back with satisfaction to what it has accomplished in its youth, and forward to an even more efficient future. One of the first of the national associations which exist in different countries for the advancement of science, its influence has been more felt than that of its successors because it is more wanted. The wealthiest country in the world, which has profited more—vastly more—by science than any other, England stands alone in the discredit of refusing the necessary expenditure for its development, and cares not that other nations should reap the harvest for which her own sons have laboured.

[1] Presidential Address to the British Association, Nottingham, 1893.

It is surely our duty not to rest satisfied with the reflection that England in the past has accomplished so much, but rather to unite and agitate in the confidence of eventual success. It is not the fault of governments, but of the nation, that the claims of science are not recognized. We have against us an overwhelming majority of the community, not merely of the ignorant, but of those who regard themselves as educated, who value science only in so far as it can be turned into money ; for we are still in great measure —in greater measure than any other—a nation of shopkeepers. Let us who are of the minority—the remnant who believe that truth is in itself of supreme value, and the knowledge of it of supreme utility—do all that we can to bring public opinion to our side, so that the century which has given Young, Faraday, Lyell, Darwin, Maxwell, and Thomson to England, may before it closes see us prepared to take our part with other countries in combined action for the full development of natural knowledge.

Last year the necessity of an imperial observatory for physical science was, as no doubt many are aware, the subject of a discussion in Section A, which derived its interest from the number of leading physicists who took part in it, and especially from the presence and active participation of the distinguished man who is at the head of the National Physical Laboratory at Berlin. The equally pressing necessity for a central institution for chemistry, on a scale commensurate with the practical importance of that science, has been insisted upon in this Association and elsewhere by distinguished chemists. As regards biology I shall have a word to say in the same direction this evening. Of these three requirements it may be that the first is the most pressing. If so, let us all, whatever branch of science we represent, unite our efforts to realize it, in the assurance that if once the claim of science to liberal public support is admitted, the rest will follow.

In selecting a subject on which to address you this evening I have followed the example of my predecessors in limiting myself to matters more or less connected with my own scientific occupations, believing that in discussing what most interests myself I should have the best chance of interesting you. The circumstance that at the last meeting of the British Association in this town, Section D assumed for the first time the title which it has since held, that of the Section of Biology, suggested to me that I might take

the word 'biology' as my starting-point, giving you some account of its origin and first use, and of the relations which subsist between biology and other branches of natural science.

Origin and Meaning of the Term 'Biology'.

The word 'biology', which is now so familiar as comprising the sum of the knowledge which has as yet been acquired concerning living nature, was unknown until after the beginning of the present century. The term was first employed by Treviranus, who proposed to himself as a life-task the development of a new science, the aim of which should be to study the forms and phenomena of life, its origin and the conditions and laws of its existence, and embodied what was known on these subjects in a book of seven volumes, which he entitled 'Biology, or the Philosophy of Living Nature'. For its construction the material was very scanty, and was chiefly derived from the anatomists and physiologists. For botanists were entirely occupied in completing the work which Linnaeus had begun and the scope of zoology was in like manner limited to the description and classification of animals. It was a new thing to regard the study of living nature as a science by itself, worthy to occupy a place by the side of natural philosophy, and it was therefore necessary to vindicate its claim to such a position. Treviranus declined to found this claim on its useful applications to the arts of agriculture and medicine, considering that to regard any subject of study in relation to our bodily wants—in other words to utility—was to narrow it, but dwelt rather on its value as a discipline and on its surpassing interest. He commends biology to his readers as a study which, above all others, 'nourishes and maintains the taste for simplicity and nobleness; which affords to the intellect ever new material for reflection, and to the imagination an inexhaustible source of attractive images.'

Being himself a mathematician as well as a naturalist, he approaches the subject both from the side of natural philosophy and from that of natural history, and desires to found the new science on the fundamental distinction between living and non-living material. In discussing this distinction, he takes as his point of departure the constancy with which the activities which manifest themselves in the

universe are balanced, emphasizing the impossibility of excluding from that balance the vital activities of plants and animals. The difference between vital and physical processes he accordingly finds, not in the nature of the processes themselves, but in their co-ordination; that is, in their adaptedness to a given purpose, and to the peculiar and special relation in which the organism stands to the external world. All of this is expressed in a proposition difficult to translate into English, in which he defines life as consisting in the reaction of the organism to external influences, and contrasts the uniformity of vital reactions with the variety of their exciting causes.[1]

The purpose which I have in view in taking you back as I have done to the beginning of the century is not merely to commemorate the work done by the wonderfully acute writer to whom we owe the first scientific conception of the science of life as a whole, but to show that this conception, as expressed in the definition I have given you as its foundation, can still be accepted as true. It suggests the *idea of organism* as that to which all other biological ideas must relate. It also suggests, although perhaps it does not express it, that *action* is not an attribute of the organism but of its essence—that if, on the one hand, protoplasm is the basis of life, life is the basis of protoplasm. Their relations to each other are reciprocal. We think of the visible structure only in connexion with the invisible process. The definition is also of value as indicating at once the two lines of inquiry into which the science has divided by the natural evolution of knowledge. These two lines may be easily educed from the general principle from which Treviranus started, according to which it is the fundamental characteristic of the organism that all that goes on in it is to the advantage of the whole. I need scarcely say that this fundamental conception of organism has at all times presented itself to the minds of those who have sought to understand the distinction between living and non-living. Without going back to the true father and founder of biology, Aristotle, we may recall with interest the language employed in relation to it by the physiologists of three hundred years ago. It was at that time expressed by the term *consensus partium*—which was defined as the

[1] 'Leben besteht in der Gleichförmigkeit der Reaktionen bei ungleichförmigen Einwirkungen der Aussenwelt.'—Treviranus, *Biologie oder Philosophie der lebenden Natur*, Göttingen, 1802, vol. i, p. 83.

concurrence of parts in action, of such a nature that each does *quod suum est*, all combining to bring about one effect 'as if they had been in secret council', but at the same time *constanti quadam naturae lege*.[1] Professor Huxley has made familiar to us how a century later Descartes imagined to himself a mechanism to carry out this *consensus*, based on such scanty knowledge as was then available of the structure of the nervous system. The discoveries of the early part of the present century relating to reflex action and the functions of sensory and motor nerves, served to realize in a wonderful way his anticipations as to the channels of influence, afferent and efferent, by which the *consensus* is maintained; and in recent times (as we hope to learn from Professor Horsley's lecture on the physiology of the nervous system) these channels have been investigated with extraordinary minuteness and success.

Whether with the old writers we speak about *consensus*, with Treviranus about *adaptation*, or are content to take *organism* as our point of departure, it means that, regarding a plant or an animal as an organism, we concern ourselves primarily with its activities or, to use the word which best expresses it, its energies. Now the first thing that strikes us in beginning to think about the activities of an organism is that they are naturally distinguishable into two kinds, according as we consider the action of the whole organism in its relation to the external world or to other organisms, or the action of the parts or organs in their relation to each other. The distinction to which we are thus led between the *internal* and *external* relations of plants and animals has of course always existed, but has only lately come into such prominence that it divides biologists more or less completely into two camps—on the one hand those who make it their aim to investigate the actions of the organism and its parts by the accepted methods of physics and chemistry, carrying this investigation as far as the conditions under which each process manifests itself will permit; on the other those who interest themselves rather in considering the place which each organism occupies, and the part which it plays in the economy of nature. It is apparent that the two lines of inquiry, although they equally relate to what the organism *does*, rather than to what it *is*, and therefore both have equal right to be

[1] Bausner, *De Consensu Partium Humani Corporis*, Amst., 1556. Praef. ad lectorem, p. 4.

included in the one great science of life, or biology, yet lead in directions which are scarcely even parallel. So marked, indeed, is the distinction, that Professor Haeckel some twenty years ago proposed to separate the study of organisms with reference to their place in nature under the designation of 'oecology', defining it as comprising 'the relations of the animal to its organic as well as to its inorganic environment, particularly its friendly or hostile relations to those animals or plants with which it comes into direct contact'.[1] Whether this term expresses it or not, the distinction is a fundamental one. Whether with the oecologist we regard the organism in relation to the world, or with the physiologist as a wonderful complex of vital energies, the two branches have this in common, that both studies fix their attention, not on stuffed animals, butterflies in cases, or even microscopical sections of the animal or plant body—all of which relate to the framework of life—but on life itself.

The conception of biology which was developed by Treviranus as far as the knowledge of plants and animals which then existed rendered possible, seems to me still to express the scope of the science. I should have liked, had it been within my power, to present to you both aspects of the subject in equal fullness; but I feel that I shall best profit by the present opportunity if I derive my illustrations chiefly from the division of biology to which I am attached—that which concerns the *internal* relations of the organism, it being my object not to specialize in either direction, but, as Treviranus desired to do, to regard it as part—surely a very important part—of the great science of nature.

The origin of life, the first transition from non-living to living, is a riddle which lies outside of our scope. No seriously-minded person, however, doubts that organized nature as it now presents itself to us has become what it is by a process of gradual perfecting or advancement, brought about by the elimination of those organisms which failed to obey the fundamental principle of adaptation which Treviranus indicated. Each step, therefore, in this evolution is a reaction to external influences, the motive of which

[1] These he identifies with 'those complicated mutual relations which Darwin designates as conditions of the struggle for existence'. Along with chorology—the distribution of animals—oecology constitutes what he calls *Relationsphysiologie*.—Haeckel, 'Entwickelungsgang u. Aufgaben der Zoologie', *Jenaische Zeitschr.* vol. v, 1869, p. 353.

is essentially the same as that by which from moment to moment the organism governs itself. And the whole process is a necessary outcome of the fact that those organisms are most prosperous which look best after their own welfare. As in that part of biology which deals with the internal relations of the organism, the interest of the individual is in like manner the sole motive by which every energy is guided. We may take what Treviranus called *selfish* adaptation—*Zweckmässigkeit für sich selber*—as a connecting link between the two branches of biological study. Out of this relation springs another which I need not say was not recognized until after the Darwinian epoch —that, I mean, which subsists between the two evolutions, that of the race and that of the individual. Treviranus, no less distinctly than his great contemporary Lamarck, was well aware that the affinities of plants and animals must be estimated according to their developmental value, and consequently that classification must be founded on development; but it occurred to no one what the real link was between descent and development; nor was it, indeed, until several years after the publication of the 'Origin' that Haeckel enunciated that 'biogenetic law' according to which the development of any individual organism is but a memory, a recapitulation by the individual of the development of the race—of the process for which Fritz Müller had coined the excellent word 'phylogenesis'; and that each stage of the former is but a transitory reappearance of a bygone epoch in its ancestral history. If, therefore, we are right in regarding ontogenesis as dependent on phylogenesis the origin of the former must correspond with that of the latter; that is, on the power which the race or the organism at every stage of its existence possesses of profiting by every condition or circumstance for its own advancement.

From the short summary of the connexion between different parts of our science you will see that biology naturally falls into three divisions, and these are even more sharply distinguished by their methods than by their subjects; namely, *Physiology*, of which the methods are entirely experimental; *Morphology*, the science which deals with the forms and structure of plants and animals, and of which it may be said that the body is anatomy, the soul, development; and finally *Oecology*, which uses all the knowledge it can obtain from the other two, but chiefly

rests on the exploration of the endless varied phenomena of animal and plant life as they manifest themselves under natural conditions. This last branch of biology—the science which concerns itself with the external relations of plants and animals to each other, and to the past and present conditions of their existence—is by far the most attractive. In it those qualities of mind which especially distinguish the naturalist find their highest exercise, and it represents more than any other branch of the subject what Treviranus termed the 'philosophy of living nature'. Notwithstanding the very general interest which several of its problems excite at the present moment I do not propose to discuss any of them, but rather to limit myself to the humbler task of showing that the fundamental idea which finds one form of expression in the world of living beings regarded as a whole—the prevalence of the best—manifests itself with equal distinctness, and plays an equally essential part in the internal relations of the organism in the great science which treats of them—Physiology.

Origin and Scope of Modern Physiology.

Just as there was no true philosophy of living nature until Darwin, we may with almost equal truth say that physiology did not exist as a science before Johannes Müller. For although the sum of his numerous achievements in comparative anatomy and physiology, notwithstanding their extraordinary number and importance, could not be compared for merit and fruitfulness with the one discovery which furnished the key to so many riddles, he, no less than Darwin, by his influence on his successors was the beginner of a new era.

Müller taught in Berlin from 1833 to 1857. During that time a gradual change was in progress in the way in which biologists regarded the fundamental problem of life. Müller himself, in common with Treviranus and all the biological teachers of his time, was a vitalist, i. e. he regarded what was then called the *vis vitalis*—the *Lebenskraft*—as something capable of being correlated with the physical forces; and as a necessary consequence held that phenomena should be classified or distinguished, according to the forces which produced them, as vital or physical, and that all those processes—that is groups or series of phenomena in living organisms—for which, in the then very imperfect knowledge

which existed, no obvious physical explanation could be found, were sufficiently explained when they were stated to be dependent on so-called vital laws. But during the period of Müller's greatest activity times were changing, and he was changing with them. During his long career as professor at Berlin he became more and more objective in his tendencies, and exercised an influence in the same direction on the men of the next generation, teaching them that it was better and more useful to observe than to philosophize; so that, although he himself is truly regarded as the last of the vitalists—for he was a vitalist to the last—his successors were adherents of what has been very inadequately designated the mechanistic view of the phenomena of life. The change thus brought about just before the middle of this century was a revolution. It was not a substitution of one point of view for another, but simply a frank abandonment of theory for fact, of speculation for experiment. Physiologists ceased to theorize because they found something better to do. May I try to give you a sketch of this era of progress?

Great discoveries as to the structure of plants and animals had been made in the course of the previous decade, those especially which had resulted from the introduction of the microscope as an instrument of research. By its aid Schwann had been able to show that all organized structures are built up of those particles of living substance which we now call cells, and recognize as the seats and sources of every kind of vital activity. Hugo Mohl, working in another direction, had given the name 'protoplasm' to a certain hyaline substance which forms the lining of the cells of plants, though no one as yet knew that it was the essential constituent of all living structures—the basis of life no less in animals than in plants. And, finally, a new branch of study—histology—founded on observations which the microscope had for the first time rendered possible, had come into existence. Bowman, one of the earliest and most successful cultivators of this new science, called it physiological anatomy[1], and justified the title by the very important inferences as to the secreting function of epithelial cells and as to the nature of muscular contraction, which he deduced from his admirable anatomical researches. From structure to function, from microscopical observation

[1] The first part of the *Physiological Anatomy* appeared in 1843. It was concluded in 1856.

to physiological experiment, the transition was natural. Anatomy was able to answer some questions, but asked many more. Fifty years ago physiologists had microscopes but had no laboratories. English physiologists—Bowman, Paget, Sharpey—were at the same time anatomists, and in Berlin Johannes Müller, along with anatomy and physiology, taught comparative anatomy and pathology. But soon that specialization which, however much we may regret its necessity, is an essential concomitant of progress, became more and more inevitable. The structural conditions on which the processes of life depend had become, if not known, at least accessible to investigation ; but very little indeed had been ascertained of the nature of the processes themselves—so little indeed that if at this moment we could blot from the records of physiology the whole of the information which had been acquired, say in 1840, the loss would be difficult to trace—not that the previously known facts were of little value, but because every fact of moment has since been subjected to experimental verification. It is for this reason that, without any hesitation, we accord to Müller and to his successors Brücke, du Bois-Reymond, Helmholtz, who were his pupils, and Ludwig, in Germany, and to Claude Bernard [1] in France, the title of founders of our science. For it is the work which they began at that remarkable time (1845-55), and which is now being carried on by their pupils or their pupils' pupils in England, America, France, Germany, Denmark, Sweden, Italy, and even in that youngest contributor to the advancement of science, Japan, that physiology has been gradually built up to whatever completeness it has at present attained.

What were the conditions which brought about this great advance which coincided with the middle of the century? There is but little difficulty in answering the question. I have already said that the change was not one of doctrine, but of method. There was, however, a leading idea in the minds of those who were chiefly concerned in bringing it about. That leading notion was, that, however complicated may be the conditions under which vital energies manifest themselves, they can be split into processes which are identical in nature with those of the non-living world,

[1] It is worthy of note that these five distinguished men were nearly contemporaries: Ludwig graduated in 1839, Bernard in 1843, the other three between those dates. Three survive—Helmholtz, Ludwig, du Bois-Reymond.

and, as a corollary to this, that the analysing of a vital process into its physical and chemical constituents so as to bring these constituents into measurable relation with physical or chemical standards, is the only mode of investigating them which can lead to satisfactory results.

There were several circumstances which at that time tended to make the younger physiologists (and all of the men to whom I have just referred were then young) sanguine, perhaps too sanguine, in the hope that the application of experimental methods derived from the exact sciences would afford solutions of many physiological problems. One of these was the progress which had been made in the science of chemistry, and particularly the discovery that many of the compounds which before had been regarded as special products of vital processes could be produced in the laboratory, and the more complete knowledge which had been thereby acquired of their chemical constitutions and relations. In like manner, the new school profited by the advances which had been made in physics, partly by borrowing from the physical laboratory various improved methods of observing the phenomena of living beings, but chiefly in consequence of the direct bearing of the crowning discovery of that epoch (that of the conservation of energy) on the discussions which then took place as to the relations between vital and physical forces; in connexion with which it may be noted that two of those who (along with Mr. Joule and your President at the last Nottingham meeting) took a prominent part in that discovery—Helmholtz and J. R. Mayer—were physiologists as much as they were physicists. I will not attempt even to enumerate the achievements of that epoch of progress. I may, however, without risk of wearying you, indicate the lines along which research at first proceeded, and draw your attention to the contrast between then and now. At present a young observer who is zealous to engage in research finds himself provided with the most elaborate means of investigation, the chief obstacle to his success being that the problems which have been left over by his predecessors are of extreme difficulty, all of the easier questions having been worked out. There were then also difficulties, but of an entirely different kind. The work to be done was in itself easier, but the means for doing it were wanting, and every investigator had to depend on his own resources. Consequently the successful men were

those who, in addition to scientific training, possessed the ingenuity to devise and the skill to carry out methods for themselves. The work by which du Bois-Reymond laid the foundation of animal electricity would not have been possible had not its author, besides being a trained physicist, known how to do as good work in a small room in the upper floor of the old University Building at Berlin as any which is now done in his splendid laboratory. Had Ludwig not possessed mechanical aptitude, in addition to scientific knowledge, he would have been unable to devise the apparatus by which he measured and recorded the variations of arterial pressure (1848), and verified the principles which Young had laid down thirty years before as to the mechanics of the circulation. Nor, lastly, could Helmholtz, had he not been a great deal more than a mere physiologist, have made those measurements of the time-relations of muscular and nervous responses to stimulation, which not only afford a solid foundation for all that has been done since in the same direction, but have served as models of physiological experiment, and as evidence that perfect work was possible and was done by capable men, even when there were no physiological laboratories.

Each of these examples relates to work done within a year or two of the middle of the century.[1] If it were possible to enter more fully on the scientific history of the time, we should, I think, find the clearest evidence, first, that the foundation was laid in anatomical discoveries, in which it is gratifying to remember that English anatomists (Allen Thomson, Bowman, Goodsir, Sharpey) took considerable share; secondly, that progress was rendered possible by the rapid advances which, during the previous decade, had been made in physics and chemistry, and the participation of physiology in the general awakening of the scientific spirit which these discoveries produced. I venture, however, to think that, notwithstanding the operation of these two causes, or rather combinations of causes, the development of our science would have been delayed had it not been for the exceptional endowments of the four or five young experimenters whose names I have mentioned,

[1] The *Untersuchungen über thierische Electricität* appeared in 1848; Ludwig's researches on the circulation, which included the first description of the 'kymograph' and served as the foundation of the 'graphic method' in 1847: Helmholtz's research on the propagation in motor nerves in 1851.

each of whom was capable of becoming a master in his own branch, and of guiding the future progress of inquiry.

Just as the affinities of an organism can be best learned from its development, so the scope of a science may be most easily judged of by the tendencies which it exhibits in its origin. I wish now to complete the sketch I have endeavoured to give of the way in which physiology entered on the career it has since followed for the last half-century, by a few words as to the influence exercised on general physiological theory by the progress of research. We have seen that no real advance was made until it became possible to investigate the phenomena of life by methods which approached more or less closely to those of the physicist, in exactitude. The methods of investigation being physical or chemical, the organism itself naturally came to be considered as a complex of such processes, and nothing more. And in particular the idea of adaptation, which, as I have endeavoured to show, is not a consequence of organism, but its essence, was in great measure lost sight of. Not, I think, because it was any more possible than before to conceive of the organism otherwise than as a working together of parts for the good of the whole, but rather that, if I may so express it, the minds of men were so occupied with new facts that they had not time to elaborate theories. The old meaning of the term 'adaptation' as the equivalent of 'design' had been abandoned, and no new meaning had yet been given to it, and consequently the word 'mechanism' came to be employed as the equivalent of 'process', as if the constant concomitance or sequence of two events was in itself a sufficient reason for assuming a mechanical relation between them. As in daily life so also in science, the misuse of words leads to misconceptions. To assert that the link between *a* and *b* is mechanical, for no better reason than that *b* always follows *a*, is an error of statement, which is apt to lead the incautious reader or hearer to imagine that the relation between *a* and *b* is understood, when in fact its nature may be wholly unknown. Whether or not at the time which we are considering, some physiological writers showed a tendency to commit this error, I do not think that it found expression in any generally accepted theory of life. It may, however, be admitted that the rapid progress of experimental investigation led to too confident anticipations, and that to some enthusiastic minds it appeared as if we were approaching within

measurable distance of the end of knowledge. Such a tendency is, I think, a natural result of every signal advance. In an eloquent Harveian oration, delivered last autumn by Dr. Bridges, it was indicated how, after Harvey's great discovery of the circulation, men were too apt to found upon it explanations of all phenomena whether of health or disease, to such an extent that the practice of medicine was even prejudicially affected by it. In respect of its scientific importance the epoch we are considering may well be compared with that of Harvey, and may have been followed by an undue preference of the new as compared with the old, but no more permanent unfavourable results have shown themselves. As regards the science of medicine we need only remember that it was during the years between 1845 and 1860 that Virchow made those researches by which he brought the processes of disease into immediate relation with the normal processes of cell-development and growth, and so, by making pathology a part of physiology, secured its subsequent progress and its influence on practical medicine. Similarly in physiology, the achievements of those years led on without any interruption or drawback to those of the following generation; while in general biology, the revolution in the mode of regarding the internal processes of the animal or plant organism which resulted from these achievements, prepared the way for the acceptance of the still greater revolution which the Darwinian epoch brought about in the views entertained by naturalists of the relations of plants and animals to each other and to their surroundings.

It has been said that every science of observation begins by going out botanizing, by which, I suppose, is meant that collecting and recording observations is the first thing to be done in entering on a new field of inquiry. The remark would scarcely be true of physiology, even at the earliest stage of its development, for the most elementary of its facts could scarcely be picked up as one gathers flowers in a wood. Each of the processes which go to make up the complex of life requires separate investigation, and in each case the investigation must consist in first splitting up the process into its constituent phenomena, and then determining their relation to each other, to the process of which they form part, and to the conditions under which they manifest themselves. It will, I think, be found that even in the simplest inquiry into the nature

of vital processes some such order as this is followed. Thus, for example, if muscular contraction be the subject on which we seek information, it is obvious that, in order to measure its duration, the mechanical work it accomplishes, the heat wasted in doing it, the electro-motive forces which it develops, and the changes of form associated with these phenomena, special modes of observation must be used for each of them, that each measurement must be in the first instance separately made, under special conditions, and by methods specially adapted to the required purpose. In the synthetic part of the inquiry the guidance of experiment must again be sought for the purpose of discriminating between apparent and real causes, and of determining the order in which the phenomena occur. Even the simplest experimental investigations of vital processes are beset with difficulties. For, in addition to the extreme complexity of the phenomena to be examined and the uncertainties which arise from the relative inconstancy of the conditions of all that goes on in the living organism, there is this additional drawback, that, whereas in the exact sciences experiment is guided by well-ascertained laws, here the only principle of universal application is that of adaptation, and that even this cannot, like a law of physics, be taken as a basis for deductions, but only as a summary expression of that relation between external exciting causes and the reactions to which they give rise, which, in accordance with Treviranus's definition, is the essential character of vital activity.

The Specific Energies of the Organism.

When in 1826 J. Müller was engaged in investigating the physiology of vision and hearing he introduced into the discussion a term, 'specific energy,' the use of which by Helmholtz[1] in his physiological writings has rendered it familiar to all students. Both writers mean by the word energy, not the 'capacity of doing work', but simply *activity*, using it in its old-fashioned meaning, that of the Greek word from which it is derived. With the qualification 'specific' it serves, perhaps, better than any other expression to indicate the way in which adaptation manifests itself. In this more extended sense the 'specific

[1] *Handb. der physiologischen Optik*, 1886, p. 233. Helmholtz uses the word in the plural—the 'energies of the nerves of special sense'.

energy' of a part or organ—whether that part be a secreting cell, a motor cell of the brain or spinal cord, or one of the photogenous cells which produce the light of the glowworm, or the protoplasmic plate which generates the discharge of the torpedo—is simply the special action which it *normally* performs, its norma or rule of action being in each instance the *interest of the organism* as a whole of which it forms part, and the exciting cause some influence outside of the excited structure, technically called a stimulus. It thus stands for a characteristic of living structures which seems to be universal. The apparent exceptions are to be found in those bodily activities which, following Bichat, we call vegetative, because they go on, so to speak, as a matter of course ; but the more closely we look into them the more does it appear that they form no exception to the general rule, that every link in the chain of living action, however uniform that action may be, is a response to an antecedent influence. Nor can it well be doubted that, as every living cell or tissue is called upon to act in the interest of the whole, the organism must be capable of influencing every part so as to regulate its action. For, although there are some instances in which the channels of this influence are as yet unknown, the tendency of recent investigations has been to diminish the number of such instances. In general there is no difficulty in determining both the nature of the central influence exercised and the relation between it and the normal function. It may help to illustrate this relation to refer to the expressive word *Auslösung* by which it has for many years been designated by German writers. This word stands for the performance of function by the 'letting off' of 'specific energies.' Carrying out the notion of 'letting off' as expressing the link between action and reaction, we might compare the whole process to the mode of working of a repeating clock (or other similar mechanism), in which case the pressure of the finger on the button would represent the external influence or stimulus, the striking of the clock, the normal reaction. And now may I ask you to consider in detail one or two illustrations of physiological reaction—of the *letting off of specific energy?*

The repeater may serve as a good example, inasmuch as it is, in biological language, a highly differentiated structure, to which a single function is assigned. So also in the living organism, we find the best examples of specific

energy where Müller found them, namely, in the most differentiated, or, as we are apt to call them, the *highest* structures. The retina, with the part of the brain which belongs to it, together constitute such a structure, and will afford us therefore the illustration we want, with this advantage for our present purpose, that the phenomena are such as we all have it in our power to observe ourselves. In the visual apparatus the principle of *normality* of reaction is fully exemplified. In the physical sense the word 'light' stands for ether vibrations, but in the sensuous or subjective sense for sensations. The swings are the stimulus, the sensations are the reaction. Between the two comes the link, the 'letting off,' which it is our business to understand. Here let us remember that the man who first recognized this distinction between the physical and the physiological was not a biologist, but a physicist. It was Young who first made clear (though his doctrine fell on unappreciative ears) that, although in vision the external influences which give rise to the sensation of light are infinitely varied, the responses need not be more than three in number, each being, in Müller's language, a 'specific energy' of some part of the visual apparatus. We speak of the organ of vision as *highly differentiated*, an expression which carries with it the suggestion of a distinction of rank between different vital processes. The suggestion is a true one; for it would be possible to arrange all those parts or organs of which the bodies of the *higher* animals consist in a series, placing at the lower end of the series those of which the functions are continuous, and therefore called vegetative; at the other, those highly specialized structures, as, e.g. those in the brain, which in response to physical light produce physiological, that is subjective, light; or, to take another instance, the so-called motor cells of the surface of the brain, which in response to a stimulus of much greater complexity produce voluntary motion. And just as in civilized society an individual is valued according to his power of doing one thing well, so the high rank which is assigned to the structure, or rather to the 'specific energy' which it represents, belongs to it by virtue of its specialization. And if it be asked how this conformity is manifested, the answer is, by the quality, intensity, duration, and extension of the response, in all which respects vision serves as so good an example, that we can readily understand how it happened that it was in this

field that the relation between response and stimulus was first clearly recognized. I need scarcely say that, however interesting it might be to follow out the lines of inquiry thus indicated, we cannot attempt it this evening. All that I can do is to mention one or two recent observations which, while they serve as illustrations, may perhaps be sufficiently novel to interest even those who are at home in the subject.

Probably every one is acquainted with some of the familiar proofs that an object is seen for a much longer period than it is actually exposed to view; that the visual reaction lasts much longer than its cause. More precise observations teach us that this response is regulated according to laws which it has in common with all the higher functions of an organism. If, for example, the cells in the brain of the torpedo are 'let off'—that is, awakened by an external stimulus—the electrical discharge, which, as in the case of vision, follows after a certain interval, lasts a certain time, first rapidly increasing to a maximum of intensity, then more slowly diminishing. In like manner, as regards the visual apparatus, we have, in the response to a sudden invasion of the eye by light, a rise and fall of a similar character. In the case of the electrical organ, and in many analogous instances, it is easy to investigate the time relations of the successive phenomena, so as to represent them graphically. Again, it is found that in many physiological reactions, the period of rising 'energy' (as Helmholtz called it) is followed by a period during which the responding structure is not only inactive, but its capacity for energizing is so completely lost that the same exciting cause which a moment before 'let off' the characteristic response is now without effect. As regards vision, it has long been believed that these general characteristics of physiological reaction have their counterpart in the visual process, the most striking evidence being that in the contemplation of a lightning flash—or, better, of an instantaneously illuminated white disk[1]—the eye seems to receive a double stroke, indicating that, although the stimulus is single and instantaneous, the response is reduplicated. The most precise of the methods we until lately possessed for investigating the wax and wane of the visual reaction, were not only difficult to carry out but

[1] The phenomenon is best seen when, in a dark room, the light of a luminous spark is thrown on to a white screen with the aid of a suitable lens.

left a large margin of uncertainty. It was therefore particularly satisfactory when M. Charpentier, of Nancy, whose merits as an investigator are perhaps less known than they deserve to be, devised an experiment of extreme simplicity which enables us, not only to observe, but to measure with great facility both phases of the reaction. It is difficult to explain even the simplest apparatus without diagrams; you will, however, understand the experiment if you will imagine that you are contemplating a disk, like those ordinarily used for colour mixing; that it is divided by two radial lines which diverge from each other at an angle of 60°; that the sector which these lines enclose is white, the rest black; that the disk revolves slowly, about once in two seconds. You then see, close to the front edge of the advancing sector, a black bar, followed by a second at the same distance from itself but much fainter. Now the scientific value of the experiment consists in this, that the angular distance of the bar from the black border is in proportion to the frequency of the revolutions—the faster the wider. If, for example, when the disk makes half a revolution in a second the distance is ten degrees, this obviously means that when light bursts into the eye, the extinction happens one-eighteenth of a second after the excitation.[1]

The fact thus demonstrated, that the visual reaction consequent on an instantaneous illumination exhibits the alternations I have described, has enabled M. Charpentier to make out another fact in relation to the visual reaction which is, I think, of equal importance. In all the instances, excepting the retina, in which the physiological response to stimulus has a definite time-limitation, and in so far resembles an explosion—in other words, in all the higher forms of specific energy, it can be shown experimentally that the process is propagated from the part first directly acted on to other contiguous parts of similar endowment. Thus in the simplest of all known phenomena of this kind, the electrical change, by which the leaf of the Dionaea plant responds to the slightest touch of its sensitive hairs, is propagated from one side of the leaf to the other, so that in the opposite lobe the response occurs after a delay which is proportional to the distance between the spot

[1] Charpentier, 'Réaction oscillatoire de la Rétine sous l'influence des excitations lumineuses,' *Archives de Physiol.*, vol. xxiv, p. 541, and *Propagation de l'action oscillatoire*, &c., p. 362.

excited and the spot observed. That in the retina there is also such propagation has not only been surmised from analogy, but inferred from certain observed facts. M. Charpentier has now been able by a method which, although simple, I must not attempt to describe, not only to prove its existence, but to measure its rate of progress over the visual field.

There is another aspect of the visual response to the stimulus of light which, if I am not trespassing too long on your patience, may, I think, be interesting to consider. As the relations between the sensations of colour and the physical properties of the light which excites them, are among the most certain and invariable in the whole range of vital reactions, it is obvious that they afford as fruitful a field for physiological investigation as those in which white light is concerned. We have on one side physical facts, that is, wave-lengths or vibration-rates ; on the other, facts in consciousness—namely, sensations of colour—so simple that notwithstanding their subjective character there is no difficulty in measuring either their intensity or their duration. Between these there are *lines of influence*, neither physical nor psychological, which pass from the former to the latter through the visual apparatus (retina, nerve, brain). It is these lines of influence which interest the physiologist. The structure of the visual apparatus affords us no clues to trace them by. The most important fact we know about them is that they must be at least three in number.

It has been lately assumed by some that vision, like every other specific energy, having been developed progressively, objects were seen by the most elementary forms of eye only in chiaroscuro, that afterwards some colours were distinguished, eventually all. As regards hearing it is so. The organ which, on structural grounds, we consider to represent that of hearing in animals low in the scale of organization—as, e.g. in the Ctenophora—has nothing to do with sound [1], but confers on its possessor the power of judging of the direction of its own movements in the water in which it swims, and of guiding these movements accordingly. In the lowest vertebrates, as, e.g. in the dogfish, although the auditory apparatus is much more complicated

[1] Verworn, 'Gleichgewicht u. Otolithenorgan', *Pflüger's Archiv*, vol. , p. 423 ; also Ewald's Researches on the Labyrinth as a Sense-organ (*Ueber das Endorgan des Nervus octavus*, Wiesbaden, 1892).

in structure, and plainly corresponds with our own, we still find the particular part which is concerned in hearing scarcely traceable. All that is provided for is that sixth sense, which the higher animals also possess, and which enables them to judge of the direction of their own movements. But a stage higher in the vertebrate series we find the special mechanisms by which we ourselves appreciate sounds beginning to appear—not supplanting or taking the place of the imperfect organ, but added to it. As regards hearing, therefore, a new function is acquired without any transformation or fusion of the old into it. We ourselves possess the sixth sense, by which we keep our balance and which serves as the guide to our bodily movements. It resides in the part of the internal ear which is called the labyrinth. At the same time we enjoy along with it the possession of the cochlea, that more complicated apparatus by which we are able to hear sounds and to discriminate their vibration-rates.

As regards vision, evidence of this kind is wanting. There is, so far as I know, no proof that visual organs which are so imperfect as to be incapable of distinguishing the forms of objects, may not be affected differently by their colours. Even if it could be shown that the least perfect forms of eye possess only the power of discriminating between light and darkness, the question whether in our own such a faculty exists separately from that of distinguishing colours is one which can only be settled by experiment. As in all sensations of colour the sensation of brightness is mixed, it is obvious that one of the first points to be determined is whether the latter represents a 'specific energy' or merely a certain combination of specific energies which are excited by colours. The question is not whether there is such a thing as white light, but whether we possess a separate faculty by which we judge of light and shade—a question which, although we have derived our knowledge of it chiefly from physical experiment, is one of eye and brain, not of wave-lengths or vibration-rates, and is therefore essentially physiological.

There is a German proverb which says, 'Bei Nacht sind alle Katzen grau.' The fact which this proverb expresses presents itself experimentally when a spectrum projected on a white surface is watched, while the intensity of the light is gradually diminished. As the colours fade away they become indistinguishable as such, the last seen being

the primary red and green. Finally they also disappear, but a grey band of light still remains, of which the most luminous part is that which before was green.[1] Without entering into details, let us consider what this tells us of the specific energy of the visual apparatus. Whether or not the faculty by which we see grey in the dark is one which we possess in common with animals of imperfectly developed vision, there seems little doubt that there are individuals of our own species who, in the fullest sense of the expression, have no eye for colour ; in whom all colour sense is absent ; persons who inhabit a world of grey, seeing all things as they might have done had they and their ancestors always lived nocturnal lives. In the theory of colour vision, as it is commonly stated, no reference is made to such a faculty as we are now discussing.

Professor Hering, whose observations as to the diminished spectrum I referred to just now, who was among the first to subject the vision of the *totally* colour-blind to accurate examination, is of opinion, on that and on other grounds, that the sensation of light and shade is a specific faculty. Very recently the same view has been advocated on a wide basis by a distinguished psychologist, Professor Ebbinghaus.[2] Happily, as regards the actual experimental results relating to both these main subjects, there seems to be a complete coincidence of observation between observers who interpret them differently. Thus the recent elaborate investigations of Captain Abney[3] (with General Festing), representing graphically the results of his measurements of the subjective values of the different parts of the diminished spectrum, as well as those of the fully illuminated spectrum as seen by the totally colour-blind, are in the closest accord with the observations of Hering, and have, moreover, been substantially confirmed in both points by the measurements of Dr. König in Helmholtz' laboratory at Berlin.[4] That observers of such eminence as the three persons whom I have mentioned, employing different methods and with a different purpose in view, and without reference to each

[1] Hering, 'Untersuch. eines total Farbenblinden,' *Pflüger's Arch.*, vol. xlix, 1891, p. 563.

[2] Ebbinghaus, 'Theorie des Farbensehens,' *Zeitschr. f. Psychol.*, vol. v, 1893, p. 145.

[3] Abney and Festing, Colour Photometry, Part III. *Phil. Trans.*, vol. clxxxiiiA, 1891, p. 531.

[4] König, 'Ueber den Helligkeitswerth der Spectralfarben bei verschiedener absoluter Intensität' *Beiträge zur Psychologie*, &c., 'Festschrift zu H. von Helmholtze 70. Geburtstage,' 1891, p. 309.

other's work, should arrive in so complicated an inquiry at coincident results, augurs well for the speedy settlement of this long-debated question. At present the inference seems to be that such a specific energy as Hering's theory of vision postulates actually exists, and that it has for associates the colour-perceiving activities of the visual apparatus, provided that these are present; but that whenever the intensity of the illumination is below the chromatic threshold—that is, too feeble to awaken these activities—or when, as in the totally colour-blind, they are wanting, it manifests itself independently; all of which can be most easily understood on such a hypothesis as has lately been suggested in an ingenious paper by Mrs. Ladd Franklin,[1] that each of the elements of the visual apparatus is made up of a central structure for the sensation of light and darkness, with collateral appendages for the sensations of colour—it being, of course, understood that this is a mere diagrammatic representation, which serves no purposes beyond that of facilitating the conception of the relation between the several 'specific energies.'

Experimental Psychology.

Resisting the temptation to pursue this subject further, I will now ask you to follow me into a region which, although closely connected with the subjects we have been considering, is beset with greater difficulties—the subject in which, under the name of Physiological or Experimental Psychology, physiologists and psychologists have of late years taken a common interest—a borderland not between fact and fancy, but between two methods of investigation of questions which are closely related, which here, though they do not overlap, at least interdigitate. It is manifest that, quite irrespectively of any foregone conclusion as to the dependence of mind on processes of which the biologist is accustomed to take cognizance, mind must be regarded as one of the 'specific energies' of the organism, and should on that ground be included in the subject-matter of physiology. As, however, our science, like other sciences, is limited not merely by its subject but also by its method, it

[1] Christine Ladd Franklin, 'Eine neue Theorie der Lichtempfindungen,' *Zeitschr. für Psychologie*, vol. iv, 1893, p. 211; see also the Proceedings of the last Psychological Congress in London, 1892.

actually takes in only so much of psychology as is experimental. Thus sensation, although it is psychological, and the investigation of its relation to the special structures by which the mind keeps itself informed of what goes on in the outside world, have always been considered to be in the physiological sphere. And it is by anatomical researches relating to the minute structure and to the development of the brain, by observation of the facts of disease, and, above all, by physiological experiment, that those changes in the ganglion cells of the brain and spinal cord which are the immediate antecedents of every kind of bodily action have been traced. Between the two—that is, between sensation and the beginning of action—there is an intervening region which the physiologist has hitherto willingly resigned to psychology, feeling his incompetence to use the only instrument by which it can be explored—that of introspection. This consideration enables us to understand the course which the new study (I will not claim for it the title of a new science, regarding it as merely a part of the great science of life) has hitherto followed, and why physiologists have been unwilling to enter on it. The study of the less complicated internal relations of the organism has afforded so many difficult problems that the most difficult of all have been deferred; so that although the psychophysical method was initiated by E. H. Weber in the middle of the present century, by investigations[1] which formed part of the work done at that epoch of discovery, and although Professor Wundt, also a physiologist, has taken a larger share in the more recent development of the new study, it is chiefly by psychologists that the researches which have given to it its importance as a new discipline have been conducted.

Although, therefore, experimental psychology has derived its methods from physical science, the result has been not so much that physiologists have become philosophers, as that philosophers have become experimental psychologists. In our own universities, in those of America, and still more in those of Germany, psychological students of mature age are to be found who are willing to place themselves in the dissecting-room side by side with beginners in anatomy, in order to acquire that exact knowledge of the framework of the organism without which no man can understand its

[1] Weber's researches were published in Wagner's *Handwörterbuch*, I think, in 1849.

working. Those, therefore, who are apprehensive lest the regions of mind should be invaded by the *insaniens sapientia* of the laboratory, may, I think, console themselves with the thought that the invaders are for the most part men who before they became laboratory workers had already given their allegiance to philosophy; their purpose being not to relinquish definitively, but merely to lay aside for a time, the weapons in the use of which they had been trained, in order to learn the use of ours. The motive that has encouraged them has not been any hope of finding an experimental solution of any of the ultimate problems of philosophy, but the conviction that, inasmuch as the relation between mental stimuli and the mental processes which they awaken is of the same order with the relation between every other vital process and its specific determinant, the only hope of ascertaining its nature must lie in the employment of the same methods of comparative measurement which the biologist uses for similar purposes. Not that there is necessarily anything scientific in mere measurement, but that measurement affords the only means by which it can be determined whether or not the same conformity in the relation between stimulus and reaction which we have accepted as the fundamental characteristic of life, is also to be found in mind, notwithstanding that mental processes have no known physical concomitants. The results of experimental psychology tend to show that it is so, and consequently that in so far the processes in question are as truly functions of organism as the contraction of a muscle, or as the changes produced in the retinal pigment by light.

I will make no attempt even to enumerate the special lines of inquiry which during the last decade have been conducted with such vigour in all parts of the world, all of them traceable to the influence of the Leipzig school; but will content myself with saying that the general purpose of these investigations has been to determine with the utmost attainable precision the nature of psychical relations. Some of these investigations begin with those simpler reactions which more or less resemble those of an automatic mechanism, proceeding to those in which the resulting action or movement is modified by the influence of auxiliary or antagonistic conditions, or changed by the simultaneous or antecedent action on the reagent of other stimuli, in all of which cases the effect can be expressed quantitatively;

others lead to results which do not so readily admit of measurement. In pursuing this course of inquiry the physiologist finds himself as he proceeds more and more the *coadjutor* of the psychologist, less and less his *director*; for whatever advantage the former may have in the mere *technique* of observation, the things with which he has to do are revealed only to introspection, and can be studied only by methods which lie outside of his sphere. I might in illustration of this refer to many recent experimental researches—such, for example, as those by which it has been sought to obtain exact data as to the physiological concomitants of pleasure and of pain, or as to the influence of weariness and recuperation, as modifiers of psychological reactions. Another outwork of the mental citadel which has been invaded by the experimental method is that of memory. Even here it can be shown that in the comparison of transitory as compared with permanent memory —as, for example, in the getting off by heart of a wholly uninteresting series of words, with subsequent oblivion and reacquisition—the labour of acquiring and reacquiring may be measured, and consequently the relation between them; and that this ratio varies according to a simple numerical law.

I think it not unlikely that the only effect of what I have said may be to suggest to some of my hearers the question, What is the use of such inquiries? Experimental psychology has, to the best of my knowledge, no technical application. The only satisfactory answer I can give is that it has exercised, and will exercise in future, a helpful influence on the science of life. Every science of observation, and each branch of it, derives from the peculiarities of its methods certain tendencies which are apt to predominate unduly. We speak of this as specialization, and are constantly striving to resist its influence. The most successful way of doing so is by availing ourselves of the counteracting influence which two opposite tendencies mutually exercise when they are simultaneous. He that is skilled in the methods of introspection naturally (if I may be permitted to say so) looks at the same thing from an opposite point of view to that of the experimentalist. It is, therefore, good that the two should so work together that the tendency of the experimentalist to imagine the existence of mechanism where none is proved to exist—of the psychologist to approach the phenomena of mind too

exclusively from the subjective side—may mutually correct and assist each other.

Phototaxis and Chemiotaxis.

Considering that every organism must have sprung from a unicellular ancestor, some have thought that unless we are prepared to admit a deferred epigenesis of mind, we must look for psychical manifestations even among the lowest animals, and that as in the protozoon all the vital activities are blended together, mind should be present among them not merely potentially but actually, though in diminished degree.

Such a hypothesis involves ultimate questions which it is unnecessary to enter upon: it will, however, be of interest in connection with our present subject to discuss the phenomena which served as a basis for it—those which relate to what may be termed the behaviour of unicellular organisms and of individual cells, in so far as these last are capable of reacting to external influences. The observations which afford us most information are those in which the stimuli employed can be easily measured, such as electrical currents, light, or chemical agents in solution.

A single instance, or at most two, must suffice to illustrate the influence of light in directing the movements of freely moving cells, or, as it is termed, phototaxis. The rod-like purple organism called by Engelmann *Bacterium photometricum*,[1] is such a light-lover that if you place a drop of water containing these organisms under the microscope, and focus the smallest possible beam of light on a particular spot in the field, the spot acts as a light trap and becomes so crowded with the little rodlets as to acquire a deep port-wine colour. If instead of making his trap of white light, he projected on the field a microscopic spectrum, Engelmann found that the rodlets showed their preference for a spectral colour which is absorbed when transmitted through their bodies. By the aid of a light trap of the same kind, the very well-known spindle-shaped and flagellate cell of Euglena can be shown to have a similar power of discriminating colour, but its preference is different. This familiar organism advances with its flagellum

[1] Engelmann, 'Bacterium photometricum,' *Onderzoek. Physiol. Lab. Utrecht*, vol. vii. p. 200; also Ueber Licht- u. Farbenperception niederster Organismen, *Pflüger's Arch.*, vol. xxix. p. 387.

forwards, the sharp end of the spindle having a red or orange eye point. Accordingly, the light it loves is again that which is most absorbed—viz., the blue of the spectrum (line F).

These examples may serve as an introduction to a similar one in which the directing cause of movement is not physical but chemical. The spectral light trap is used in the way already described; the organisms to be observed are not coloured, but bacteria of that common sort which twenty years ago we used to call *Bacterium termo*, and which is recognized as the ordinary determining cause of putrefaction. These organisms do not care for light, but are great oxygen-lovers. Consequently, if you illuminate with your spectrum a filament of a confervoid alga, placed in water containing bacteria, the assimilation of carbon and consequent disengagement of oxygen is most active in the part of the filament which receives the red rays (B to C). To this part, therefore, where there is a dark band of absorption, the bacteria which want oxygen are attracted in crowds. The motive which brings them together is their desire for oxygen. Let us compare other instances in which the source of attraction is food.

The plasmodia of the myxomycetes, particularly one which has been recently investigated by Mr. Arthur Lister,[1] may be taken as a typical instance of what may be called the chemical allurement of living protoplasm. In this organism, which in the active state is an expansion of labile living material, the delicacy of the reaction is comparable to that of the sense of smell in those animals in which the olfactory organs are adapted to an aquatic life. Just as, for example, the dogfish is attracted by food which it cannot see, so the plasmodium of Badhamia becomes aware, as if it smelled it, of the presence of its food—a particular kind of fungus. I have no diagram to explain this, but will ask you to imagine an expansion of living material, quite structureless, spreading itself along a wet surface; that this expansion of transparent material is bounded by an irregular coast-line; and that somewhere near the coast there has been placed a fragment of the material on which the Badhamia feeds. The presence of this bit of *Stereum* produces an excitement at the part of the plasmodium next to it. Towards this centre of activity

[1] Lister, 'On the Plasmodium of *Badhamia utricularis*, &c.,' *Annals of Botany*, No. 5, June 1888.

streams of living material converge. Soon the afflux leads to an outgrowth of the plasmodium, which in a few minutes advances towards the desired fragment, envelopes, and incorporates it.

May I give you another example also derived from the physiology of plants? Very shortly after the publication of Engelmann's observations of the attraction of bacteria by oxygen, Pfeffer made the remarkable discovery that the movements of the antherozoids of ferns and of mosses are guided by impressions derived from chemical sources, by the allurement exercised upon them by certain chemical substances in solution—in one of the instances mentioned by sugar, in the other by an organic acid. The method consisted in introducing the substance to be tested, in any required strength, into a minute capillary tube closed at one end, and placing it under the microscope in water inhabited by antherozoids, which thereupon showed their predilection for the substance, or the contrary, by its effect on their movements. In accordance with the principle followed in experimental psychology, Pfeffer[1] made it his object to determine, not the relative effects of different doses, but the smallest perceptible increase of dose which the organism was able to detect, with this result—that, just as in measurements of the relation between stimulus and reaction in ourselves we find that the sensational value of a stimulus depends, not on its absolute intensity, but on the ratio between that intensity and the previous excitation, so in this simplest of vital reagents the same so-called psycho-physical law manifests itself. It is not, however, with a view to this interesting relation that I have referred to Pfeffer's discovery, but because it serves as a centre around which other phenomena, observed alike in plants and animals, have been grouped. As a general designation of reactions of this kind Pfeffer devised the term Chemotaxis, or, as we in England prefer to call it, Chemiotaxis. Pfeffer's contrivance for chemiotactic testing was borrowed from the pathologists, who have long used it for the purpose of determining the relation between a great variety of chemical compounds or products, and the colourless corpuscles of the blood. I need, I am sure, make no apology for referring to a question which, although purely pathological, is of very great biological interest—

[1] Pfeffer, *Untersuch. a. d. botan. Institute zu Tübingen*, vol. i, part 3, 1884.

the theory of the process by which, not only in man, but also, as Metschnikoff has strikingly shown, in animals far down in the scale of development, the organism protects itself against such harmful things as, whether particulate or not, are able to penetrate its framework. Since Cohnheim's great discovery in 1867 we have known that the central phenomenon of what is termed by pathologists *inflammation* is what would now be called a chemiotactic one; for it consists in the gathering together, like that of vultures to a carcase, of those migratory cells which have their home in the blood stream and in the lymphatic system, to any point where the living tissue of the body has been injured or damaged, as if the products of disintegration which are set free where such damage occurs were attractive to them.

The fact of chemiotaxis, therefore, as a constituent phenomenon of the process of inflammation, was familiar in pathology long before it was understood. Cohnheim himself attributed it to changes in the channels along which the cells moved, and this explanation was generally accepted, though some writers, at all events, recognized its incompleteness. But no sooner was Pfeffer's discovery known than Leber,[1] who for years had been working at the subject from the pathological side, at once saw that the two processes were of similar nature. Then followed a variety of researches of great interest by which the importance of chemiotaxis in relation to the destruction of disease-producing microphytes was proved, by that of Buchner[2] on the chemical excitability of leucocytes being among the most important. Much discussion has taken place, as many present are aware, as to the kind of wandering cells, or leucocytes, which in the first instance attack morbific microbes, and how they deal with them. The question is not by any means decided. It has, however, I venture to think, been conclusively shown that the process of destruction is a chemical one, that the destructive agent has its source in the chemiotactic cells—that is, cells which act under the orders of chemical stimuli. Two Cambridge observers, Messrs. Kanthack and Hardy,[3] have

[1] Leber, 'Die Anhäufung der Leucocyten am Orte des Entzündungsreizes,' &c. *Die Entstehung der Entzündung*, &c., pp. 423–464, Leipzig, 1891.

[2] Buchner, 'Die chem. Reizbarkeit der Leucocyten,' &c., *Berliner klin. Woch.*, 1890, No 17.

[3] Kanthack and Hardy, 'On the Characters and Behaviour of the

lately shown that, in the particular instance which they have investigated, the cells which are most directly concerned in the destruction of morbific *bacilli*, although chemiotactic, do not possess the power of incorporating either bacilli or particles of any other kind. While, therefore, we must regard the relation between the process of devitalizing and that of incorporating as not yet sufficiently determined, it is now no longer possible to regard the latter as essential to the former.

There seems, therefore, to be very little doubt that chemiotactic cells are among the agents by which the human or animal organism protects itself against infection. There are, however, many questions connected with this action which have not yet been answered. The first of these are chemical ones—that of the nature of the attractive substance and that of the process by which the living carriers of infection are destroyed. Another point to be determined is how far the process admits of adaptation to the particular infection which is present in each case, and to the state of liability or immunity of the infected individual. The subject is therefore of great complication. None of the points I have suggested can be settled by experiments in glass tubes such as I have described to you. These serve only as indications of the course to be followed in much more complicated and difficult investigations—when we have to do with acute diseases as they actually affect ourselves or animals of similar liabilities to ourselves, and find ourselves face to face with the question of their causes.

It is possible that many members of the Association are not aware of the unfavourable—I will not say discreditable—position that this country at present occupies in relation to the scientific study of this great subject—the causes and mode of prevention of infectious diseases. As regards administrative efficiency in matters relating to public health England was at one time far ahead of all other countries, and still retains its superiority; but as regards scientific knowledge we are, in this subject as in others, content to borrow from our neighbours. Those who desire either to learn the methods of research or to carry out scientific inquiries have to go to Berlin, to Munich, to Breslau, or to the Pasteur Institute in Paris to obtain what England ought long ago to have provided. For to us, from the

Wandering Cells of the Frog,' *Proceedings of the Royal Society*, vol. lii, p. 267.

spread of our race all over the world, the prevention of acute infectious diseases is more important than to any other nation. At the beginning of this address I urged the claims of pure science. If I could, I should feel inclined to speak even more strongly of the application of science to the discovery of the causes of acute diseases. May I express the hope that the effort which is now being made to establish in England an Institution for this purpose not inferior in efficiency to those of other countries, may have the sympathy of all present? And now may I ask your attention for a few moments more to the subject that more immediately concerns us?

Conclusion.

The purpose which I have had in view has been to show that there is one principle—that of adaptation—which separates biology from the exact sciences, and that in the vast field of biological inquiry the end we have is not merely, as in natural philosophy, to investigate the relation between a phenomenon and the antecedent and concomitant conditions on which it depends, but to possess this knowledge in constant reference to the interest of the organism. It may perhaps be thought that this way of putting it is too teleological, and that in taking, as it were, as my text this evening so old-fashioned a biologist as Treviranus, I am yielding to a retrogressive tendency. It is not so. What I have desired to insist on is that *organism* is a fact which encounters the biologist at every step in his investigations; that in referring it to any general biological principle, such as adaptation, we are only referring it to itself, not explaining it; that no explanation will be attainable until the conditions of its coming into existence can be subjected to experimental investigation so as to correlate them with those of processes in the non-living world.

Those who were present at the meeting of the British Association at Liverpool will remember that then, as well as at some subsequent meetings, the question whether the conditions necessary for such an inquiry could be realized was a burning one. This is no longer the case. The patient endeavours which were made about that time to obtain experimental proof of what was called *abiogenesis*, although they conduced materially to that better know-

ledge which we now possess of the conditions of life of bacteria, failed in the accomplishment of their purpose. The question still remains undetermined; it has, so to speak, been adjourned *sine die*. The only approach to it lies at present in the investigation of those rare instances in which, although the relations between a living organism and its environment ceases as a watch stops when it has not been wound, these relations can be re-established—the process of life re-awakened—by the application of the required stimulus.

I was also desirous to illustrate the relation between physiology and its two neighbours on either side, natural philosophy (including chemistry) and psychology. As regards the latter I need add nothing to what has already been said. As regards the former, it may be well to notice that although physiology can never become a mere branch of applied physics or chemistry, there are parts of physiology wherein the principles of these sciences may be applied directly. Thus, in the beginning of the century, Young applied his investigations as to the movements of liquids in a system of elastic tubes, directly to the phenomena of the circulation; and a century before, Borelli successfully examined the mechanisms of locomotion and the action of muscles, without reference to any, excepting mechanical principles. Similarly, the foundation of our present knowledge of the process of nutrition was laid in the researches of Bidder and Schmidt, in 1851, by determinations of the weight and composition of the body, the daily gain of weight by food or oxygen, the daily loss by the respiratory and other discharges, all of which could be accomplished by chemical means. But in by far the greater number of physiological investigations, both methods (the physical or chemical and the physiological) must be brought to bear on the same question—to co-operate for the elucidation of the same problem. In the researches, for example, which during several years have occupied Professor Bohr, of Copenhagen, relating to the exchange of gases in respiration, he has shown that factors purely physical—namely, the partial pressures of oxygen and carbon dioxide in the blood which flows through the pulmonary capillaries—are, so to speak, interfered with in their action by the 'specific energy' of the pulmonary tissue, in such a way as to render this fundamental process, which, since Lavoisier, has justly been regarded

as one of the most important in physiology, much more complicated than we for a long time supposed it to be. In like manner Heidenhain has proved that the process of lymphatic absorption, which before we regarded as dependent on purely mechanical causes—i.e. differences of pressure—is in great measure due to the specific energy of cells, and that in various processes of secretion the principal part is not, as we were inclined not many years ago to believe, attributable to liquid diffusion, but to the same agency. I wish that there had been time to have told you something of the discoveries which have been made in this particular field by Mr. Langley, who has made the subject of 'specific energy' of secreting-cells his own. It is in investigations of this kind, of which any number of examples could be given, in which vital reactions mix themselves up with physical and chemical ones so intimately that it is difficult to draw the line between them, that the physiologist derives most aid from whatever chemical and physical training he may be fortunate enough to possess.

There is, therefore, no doubt as to the advantages which physiology derives from the exact sciences. It could scarcely be averred that they would benefit in anything like the same degree from closer association with the science of life. Nevertheless, there are some points in respect of which that science may have usefully contributed to the advancement of physics or of chemistry. The discovery of Graham as to the characters of colloid substances, and as to the diffusion of bodies in solution through membranes, would never have been made had not Graham 'ploughed', so to speak, 'with our heifer'. The relations of certain colouring matters to oxygen and carbon dioxide would have been unknown, had no experiments been made on the respiration of animals and the assimilative process in plants; and, similarly, the vast amount of knowledge which relates to the chemical action of ferments must be claimed as of physiological origin. So also there are methods, both physical and chemical, which were originally devised for physiological purposes. Thus the method by which meteorological phenomena are continuously recorded graphically, originated from that used by Ludwig (1847) in his 'Researches on the Circulation'; the mercurial pump, invented by Lothar Meyer, was perfected in the physiological laboratories of Bonn and Leipzig; the rendering the galvanometer needle aperiodic by damping was

first realized by du Bois-Reymond—in all of which cases invention was prompted by the requirements of physiological research.

Let me conclude with one more instance of a different kind, which may serve to show how, perhaps, the wonderful ingenuity of contrivance which is displayed in certain organized structures—the eye, the ear, or the organ of voice—may be of no less interest to the physicist than to the physiologist. Johannes Müller, as is well known, explained the compound eye of insects on the theory that an erect picture is formed on the convex retina by the combination of pencils of light, received from different parts of the visual field through the eyelets (ommatidia) directed to them. Years afterwards it was shown that in each eyelet an image is formed which is reversed. Consequently, the mosaic theory of Müller was for a long period discredited on the ground that an erect picture could not be made up of 'upside-down' images. Lately the subject has been reinvestigated, with the result that the mosaic theory has regained its authority. Professor Exner[1] has proved photographically that behind each part of the insect's eye an erect picture is formed of the objects towards which it is directed. There is, therefore, no longer any difficulty in understanding how the whole field of vision is mapped out as consistently as it is imagined on our own retina, with the difference, of course, that the picture is erect. But behind this fact lies a physical question—that of the relation between the erect picture which is photographed and the optical structure of the crystal cones which produce it—a question which, although we cannot now enter upon it, is quite as interesting as the physiological one.

With this history of a theory which, after having been for thirty years disbelieved, has been reinstated by the fortunate combination of methods derived from the two sciences, I will conclude. It may serve to show how, though physiology can never become a part of natural philosophy, the questions we have to deal with are cognate. Without forgetting that every phenomenon has to be regarded with reference to its useful purpose in the organism, the aim of the physiologist is not to inquire into final causes, but to investigate processes. His question is ever How, rather than Why.

[1] Exner, *Die Physiologie der facettirten Augen von Krebsen u. Insecten*, Leipzig, 1891.

May I illustrate this by a simple, perhaps too trivial, story, which derives its interest from its having been told of the childhood of one of the greatest natural philosophers of the present century?[1] He was even then possessed by that insatiable curiosity which is the first quality of the investigator; and it is related of him that his habitual question was 'What is the *go* of it?' and if the answer was unsatisfactory, 'What is the particular go of it?' That North Country boy became Professor Clerk Maxwell. The questions he asked are those which in our various ways we are all trying to answer.

[1] *Life of Clerk Maxwell* (Campbell and Garnett), p 28.

LUDWIG AND MODERN PHYSIOLOGY [1]

I. Introduction.

The death of any discoverer—of any one who has added largely to the sum of human knowledge, affords a reason for inquiring what his work was and how he accomplished it. This inquiry has interest even when the work has been completed in a few years and has been limited to a single line of investigation—much more when the life has been associated with the origin and development of a new science and has extended over half a century.

The Science of Physiology as we know it came into existence fifty years ago with the beginning of the active life of Ludwig, in the same sense that the other great branch of Biology, the Science of Living Beings, as we now know it, came into existence with the appearance of the *Origin of Species*. In the order of time Physiology had the advantage, for the new Physiology was accepted some ten years before the Darwinian epoch. Notwithstanding, the content of the Science is relatively so unfamiliar, that before entering on the discussion of the life and work of the man who, as I shall endeavour to show, had a larger share in founding it than any of his contemporaries, it is necessary to define its limits and its relations to other branches of knowledge.

The word Physiology has in modern times changed its meaning. It once comprehended the whole knowledge of Nature. Now it is the name for one of the two Divisions of the Science of Life. In the progress of investigation the study of that Science has inevitably divided itself into two: *Ontology* [2], the Science of Living Beings; *Physiology*, the Science of Living Processes, and thus, inasmuch as Life consists in processes, of Life itself. Both strive to understand the complicated relations and endless varieties which

[1] Founded upon a lecture delivered at the Royal Institution, Jan. 24, 1896. (Article in *Science Progress*, March, 1896).

[2] I do not forget that this word is ordinarily used in another sense. Its suitability is my excuse for employing it.

present themselves in living Nature, but by different methods. Both refer to general principles, but they are of a different nature.

To the *Ontologist*, the student of Living Beings, Plants or Animals, the great fact of Evolution, namely, that from the simplest beginning our own organism, with its infinite complication of parts and powers, no less than that of every animal and plant, unfolds the plan of its existence—taken with the observation that that small beginning was, in all excepting the lowest forms, itself derived from two parents, equally from each—is the basis from which his study and knowledge of the world of living beings takes its departure. For on Evolution and Descent the explorer of the forms, distribution and habits of animals and plants has, since the Darwinian epoch, relied with an ever-increasing certainty, and has found in them the explanation of every phenomenon, the solution of every problem relating to the subject of his inquiry. Nor could he wish for a more secure basis. Whatever doubts or misgivings exist in the minds of 'non-biologists' in relation to it, may be attributed partly to the association with the doctrine of Evolution of questions which the true naturalist regards as transcendental ; partly to the perversion or weakening of meaning which the term has suffered in consequence of its introduction into the language of common life, and particularly to the habit of applying it to any kind of progress or improvement, anything which from small beginnings *gradually* increases. But, provided we limit the term to its original sense—the Evolution of a living being from its germ by a *continuous* not a gradual process, there is no conception which is more free from doubt either as to its meaning or reality. It is inseparable from that of Life itself, which is but the *unfolding* of a predestined harmony, of a prearranged consensus and synergy of parts.

The other branch of Biology, that with which Ludwig's name is associated, deals with the same facts in a different way. While Ontology regards animals and plants as individuals and in relation to other individuals, Physiology considers the processes themselves of which life is a complex. This is the most obvious distinction, but it is subordinate to the fundamental one, namely, that while Ontology has for its basis laws which are in force only in its own province, those of Evolution, Descent, and Adaptation, we Physiologists, while accepting these as true, found nothing

upon them, using them only as guides to discovery, not for the purpose of explanation. Purposive Adaptation, for example, serves as a clue, by which we are constantly guided in our exploration of the tangled labyrinth of vital processes. But when it becomes our business to explain these processes—to say how they are brought about—we refer them not to biological principles of any kind, but to the Universal Laws of Nature. Hence it happens that with reference to each of these processes, our inquiry is rather how it occurs than why it occurs.

It has been well said that the Natural Sciences are the children of necessity. Just as the other Natural Sciences owed their origin to the necessity of acquiring that control over the forces of Nature without which life would scarcely be worth living, so Physiology arose out of human suffering and the necessity of relieving it. It sprang indeed out of Pathology. It was suffering that led us to know, as regards our own bodies, that we had internal as well as external organs; and probably one of the first generalizations which arose out of this knowledge was, that 'if one member suffer all the members suffer with it'—that all work together for the good of the whole. In earlier times the *good* which was thus indicated was associated in men's minds with human welfare exclusively. But it was eventually seen that Nature has no less consideration for the welfare of those of her products which to us seem hideous or mischievous, than for those which we regard as most useful to man or most deserving of his admiration. It thus became apparent that the good in question could not be human exclusively, but as regards each animal *its own good*—and that in the organized world the existence and life of every species is brought into subordination to one purpose—its own success in the struggle for existence.[1]

From what has preceded it may be readily understood that in Physiology, Adaptation takes a more prominent

[1] I am aware that in thus stating the relation between adaptation and the struggle for existence, I may seem to be reversing the order followed by Mr. Darwin, insomuch as he regarded the survival of organisms which are fittest for their place in Nature, and of parts which are fittest for their place in the organism, as the agency by which adaptedness is brought about. However this may be expressed, it cannot be doubted that fitness is an essential property of organisms. Living beings are the only things in Nature which by virtue of evolution and descent are able to adapt themselves to their surroundings. It is therefore only so far as *organism* (with all its attributes) is presupposed, that the dependence of adaptation on survival is intelligible.

part than Evolution or Descent. In the prescientific period adaptation was everything. The observation that any structure or arrangement exhibited marks of adaptation to a useful purpose was accepted not merely as a guide in research, but as a full and final explanation. Of an organism or organ which perfectly fulfilled in its structure and working the end of its existence, nothing further required to be said or known. Physiologists of the present day recognize as fully as their predecessors that perfection of contrivance which displays itself in all living structures, the more exquisitely the more minutely they are examined. No one, for example, has written more emphatically on this point than did Ludwig. In one of his discourses, after showing how Nature exceeds the highest standard of human attainment—how she fashions as it were out of nothing and without tools, instruments of a perfection which the human artificer cannot reach, though provided with every suitable material—wood, brass, glass, india-rubber—he gives the organ of sight as a signal example, referring amongst its other perfections to the rapidity with which the eye can be fixed on numerous objects in succession, and the instantaneous and unconscious estimates which we are able to form of the distances of objects, each estimate involving a process of arithmetic which no calculating machine could effect in the time.[1] In another discourse—that given at Leipzig when he entered on his professorship in 1865, he remarks that when in our researches into the finer mechanism of an organ we at last come to understand it, we are humbled by the recognition 'that the human inventor is but a blunderer compared with the unknown Master of the animal creation.'[2]

Some readers will perhaps remember how one of the most brilliant of philosophical writers, in a discourse to the British Association delivered a quarter of a century ago, averred on the authority of a great physiologist that the eye, regarded as an optical instrument, was so inferior a production that if it were the work of a mechanician it

[1] I summarize here from a very interesting lecture entitled 'Leid und Freude in der Naturforschung', published in the *Gartenlaube* (Nos. 22 and 23) in 1870.

[2] The sentence, of which the words in inverted commas form a part, is as follows: 'Wenn uns endlich die Palme gereicht wird, wenn wir ein Organ in seinem Zusammenhang begreifen, so wird unser stolzes Gattungsbewusstsein durch die Erkenntniss niedergedrückt, dass der menschliche Erfinder ein Stümper gegen den unbekannten Meister der thierischen Schöpfung sei.

would be unsaleable. Without criticizing or endeavouring to explain this paradox, I may refer to it as having given the countenance of a distinguished name to a misconception which I know exists in the minds of many persons, to the effect that the scientific physiologist is more or less blind to the evidence of design in creation. On the contrary, the view taken by Ludwig, as expressed in the words I have quoted, is that of all physiologists. The disuse of the teleological expressions which were formerly current does not imply that the indications of contrivance are less appreciated, for, on the contrary, we regard them as more characteristic of organism as it presents itself to our observation than any other of its endowments. But, if I may be permitted to repeat what has been already said, we use the evidences of adaptation differently .We found no explanation on this or any other biological principle, but refer all the phenomena by which these manifest themselves, to the simpler and more certain physical laws of the Universe.

Why must we take this position? First, because it is a general rule in investigations of all kinds, to explain the more complex by the more simple. The material Universe is manifestly divided into two parts, the living and the non-living. We may, if we like, take the living as our Norma, and say to the physicists, You must come to us for laws, you must account for the play of energies in universal nature by referring them to Evolution, Descent, Adaptation. Or we may take these words as true expressions of the mutual relations between the phenomena and processes peculiar to living beings, using for the explanation of the processes themselves the same methods which we should employ if we were engaged in the investigation of analogous processes going on independently of life. Between these two courses there seems to me to be no third alternative, unless we suppose that there are two material Universes, one to which the material of our bodies belongs, the other comprising everything that is not either plant or animal.

The second reason is a practical one. We should have to go back to the time which I have ventured to call prescientific, when the world of life and organization was supposed to be governed exclusively by its own laws. The work of the past fifty years has been done on the opposite principle, and has brought light and clearness where there was before obscurity and confusion. All this progress we should have to repudiate, but this would not

be all. We should have to forgo the prospect of future advance. Whereas by holding on our present course, gradually proceeding from the more simple to the more complex, from the physical to the vital, we may confidently look forward to extending our knowledge considerably beyond its present limits.

A no less brilliant writer than the one already referred to, who is also no longer with us, asserted that mind was a secretion of the brain in the same sense that bile is a secretion of the liver, or urine that of the kidney; and many people have imagined this to be the necessary outcome of a too mechanical way of looking at vital phenomena, and that physiologists, by a habit of adhering strictly to their own method, have failed to see that the organism presents problems to which this method is not applicable, such, e.g. as the origin of the organism itself, or the origin and development in it of the mental faculty. The answer to this suggestion is that these questions are approached by physiologists only in so far as they are approachable. We are well aware that our business is with the unknown knowable, not with the transcendental.

During the last twenty years there has been a considerable forward movement in Physiology in the psychological direction, partly dependent on discoveries as to the localization of the higher functions of the nervous system, partly on the application of methods of measurement to the concomitant phenomena of psychical processes. And these researches have brought us to the very edge of a region which cannot be explored by our methods—where measurements of time or of space are no longer possible. In approaching this limit, the physiologist is liable to fall into two mistakes—on the one hand, that of passing into the transcendental without knowing it; on the other, that of assuming that what he does not know is not knowledge. The former of these risks seems to me of little moment; first, because the limits of natural knowledge in the psychological direction have been well defined by the best writers, as, e.g. by du Bois-Reymond in his well-known essay 'On the Limits of Natural Knowledge',[1] but chiefly because the investigator who knows what he is about is arrested *in limine* by the impossibility of applying the experimental method to questions beyond its scope. The other mistake is chiefly fallen into by careless thinkers who, while they

[1] *Ueber die Grenzen des Naturerkennens.* Reden, Leipzig, 1886.

object to the employment of intuition even in regions where intuition is the only method by which anything can be learned, attempt to describe and define mental processes in mechanical terms, assigning to these terms meanings which science does not recognize, and thus slide into a kind of speculation which is as futile as it is unphilosophical.

II. Ludwig as Investigator and Teacher.

The uneventful history of Ludwig's life—how early he began his investigation of the anatomy and function of the kidneys, how he became just fifty years ago titular Professor at Marburg, in the small University of his native State, Hesse Cassel; how in 1849 he removed to Zürich as actual Professor and thereupon married; how he was six years later promoted to Vienna, has already been admirably related by Dr. Stirling.[1] In 1865, after twenty years of professorial experience, but still in the prime of life and, as it turned out, with thirty years of activity still before him, he accepted the Chair of Physiology at Leipzig. His invitation to that great University was by far the most important occurrence in his life, for the liberality of the Saxon Government, and particularly the energetic support which he received from the enlightened Minister, v. Falkenstein, enabled him to accomplish for Physiology what had never before been attempted on an adequate scale. No sooner had he been appointed, than he set himself to create what was then essential to the progress of the Science—a great Observatory, arranged not as a museum, but much more like a physical and chemical laboratory, provided with all that was needed for the application of exact methods of research to the investigation of the processes of Life. The idea which he had ever in view, and which he carried into effect during the last thirty years of his life with signal success, was to unite his life-work as an investigator with the highest kind of teaching. Even at Marburg and at Zürich he had begun to form a *School*; for already men nearly of his own age had rallied round him. Attracted in the first instance by his early discoveries, they were held by the force of his character, and became permanently associated with him in his work as his loyal friends and followers—in the highest sense his *scholars*. If, therefore, we speak of Ludwig as one of the greatest *teachers* of

[1] See *Science Progress*, vol. iv, Nov. 1895.

Science the world has seen, we have in mind his relation to the men who ranged themselves under his leadership in the building up of the science of Physiology, without reference to his function as an ordinary academical teacher.

Of this relation we can best judge by the careful perusal of the numerous biographical memoirs which have appeared since his death, more particularly those of Professor His[1] (Leipzig), of Professor Kronecker[2] (Bern), who was for many years his coadjutor in the Institute, of Professor v. Fick[3] (Würzburg), of Professor v. Kries[4] (Freiburg), of Professor Mosso[5] (Turin), of Professor Fano[6] (Florence), of Professor Tigerstedt[7] (Upsala), of Professor Stirling[8] in England. With the exception of Fick, whose relations with Ludwig were of an earlier date, and of his colleague in the Chair of Anatomy, all of these distinguished teachers were at one time workers in the Leipzig Institute. All testify their love and veneration for the master, and each contributes some striking touches to the picture of his character.

All Ludwig's investigations were carried out with his scholars. He possessed a wonderful faculty of setting each man to work at a problem suited to his talent and previous training, and this he carried into effect by associating him with himself in some research which he had either in progress or in view. During the early years of the Leipzig period, all the work done under his direction was published in the well-known volumes of the *Arbeiten*, and subsequently in the *Archiv für Anat. und Physiologie* of du Bois-Reymond. Each 'Arbeit' of the laboratory appeared in print under the name of the scholar who operated with his master in its production, but the scholar's part in the work done varied according to its nature and his ability. Sometimes, as v. Kries says, he sat on the window-sill while Ludwig, with the efficient help of his

[1] His, 'Karl Ludwig und Karl Thiersch.' *Akademische Gedächtnissrede*, Leipzig, 1895.

[2] Kronecker, 'Carl Friedrich Wilhelm Ludwig.' *Berliner klin. Wochensch.*, 1895, no. 21.

[3] A. Fick, 'Karl Ludwig.' *Nachruf. Biographische Blätter*, Berlin, vol. i, pt. 3.

[4] v. Kries, 'Carl Ludwig.' Freiburg i. B., 1895.

[5] Mosso, 'Karl Ludwig.' *Die Nation*, Berlin, nos. 38, 39.

[6] Fano, 'Per Carlo Ludwig Commemorazione.' *Clinica Moderna*, Florence, i, no. 7.

[7] Tigerstedt, 'Karl Ludwig.' *Denkrede. Biographische Blätter*, Berlin, vol. i, pt. 3.

[8] Stirling, *loc. cit.*

laboratory assistant Salvenmoser, did the whole of the work. In all cases Ludwig not only formulated the problem, but indicated the course to be followed in each step of the investigation, calling the worker, of course, into counsel. In the final working up of the results he always took a principal part, and often wrote the whole paper. But whether he did little or much, he handed over the whole credit of the performance to his coadjutor. This method of publication has no doubt the disadvantage that it leaves it uncertain what part each had taken: but it is to be remembered that this drawback is unavoidable whenever master and scholar work together, and is outweighed by the many advantages which arise from this mode of co-operation. The instances in which any uncertainty can exist in relation to the real authorship of the Leipzig work are exceptional. The well-informed reader does not need to be told that Mosso or Schmidt, Brunton or Gaskell, Stirling or Wooldridge were the authors of their papers in a sense very different from that in which the term could be applied to some others of Ludwig's pupils. On the whole the plan must be judged of by the results. It was by working with his scholars that Ludwig trained them to work afterwards by themselves; and thereby accomplished so much more than other great teachers have done.

I do not think that any of Ludwig's contemporaries could be compared to him in respect of the wide range of his researches. In a science distinguished from others by the variety of its aims, he was equally at home in all branches, and was equally master of all methods, for he recognized that the most profound biological question can only be solved by combining anatomical, physical, and chemical inquiries. It was this consideration which led him in planning the Leipzig Institute to divide it into three parts, experimental (in the more restricted sense), chemical, and histological. Well aware that it was impossible for a man who is otherwise occupied to maintain his familiarity with the technical details of Histology and Physiological Chemistry, he placed these departments under the charge of younger men capable of keeping them up to the rapidly advancing standard of the time, his relations with his coadjutors being such that he had no difficulty in retaining his hold of the threads of the investigation to which these special lines of inquiry were contributory.

It is scarcely necessary to say that as an experimenter Ludwig was unapproachable. The skill with which he carried out difficult and complicated operations, the care with which he worked, his quickness of eye and certainty of hand were qualities which he had in common with great surgeons. In employing animals for experiment he strongly objected to rough and ready methods, comparing them to 'firing a pistol into a clock to see how it works'. Every experiment ought, he said, to be carefully planned and meditated on beforehand, so as to accomplish its scientific purpose and avoid the infliction of pain. To ensure this he performed all operations himself, only rarely committing the work to a skilled coadjutor.

His skill in anatomical work was equally remarkable. It had been acquired in early days, and appeared throughout his life to have given him very great pleasure, for Mosso tells how, when occupying the room adjoining that in which Ludwig was working, as he usually did by himself, he heard the outbursts of glee which accompanied each successful step in some difficult anatomical investigation.

Let us now examine more fully the part which Ludwig played in the revolution of ideas as to the nature of vital processes which, as we have seen, took place in the middle of the present century.

Although, as we shall see afterwards, there were many men who, before Ludwig's time, investigated the phenomena of life from the physical side, it was he and the contemporaries who were associated with him who first clearly recognized the importance of the principle that vital phenomena *can only be understood by comparison with their physical counterparts*, and foresaw that in this principle the future of Physiology was contained as in a nutshell. Feeling strongly the fruitlessness and unscientific character of the doctrines which were then current, they were eager to discover chemical and physical relations in the processes of life. In Ludwig's intellectual character this eagerness expressed his dominant motive. Notwithstanding that his own researches had in many instances proved that there are important functions and processes in the animal organism which have no physical or chemical analogues, he never swerved either from the principle or from the method founded upon it.

Although Ludwig was strongly influenced by the rapid

progress which was being made in scientific discovery at the time that he entered on his career, he derived little from his immediate predecessors in his own science. He is sometimes placed among the pupils of the great comparative anatomist and physiologist, J. Müller. This, however, is a manifest mistake, for Ludwig did not visit Berlin until 1847, when Müller was nearly at the end of his career. At that time he had already published researches of the highest value (those on the Mechanism of the Circulation and on the Physiology of the Kidney), and had set forth the line in which he intended to direct his investigations. The only earlier physiologist with whose work that of Ludwig can be said to be in real continuity was E. H. Weber, whom he succeeded at Leipzig, and strikingly resembled in his way of working. For Weber, Ludwig expressed his veneration more unreservedly than for any other man excepting perhaps Helmholtz, regarding his researches as the foundation on which he himself desired to build. Of his colleagues at Marburg he was indebted in the first place to the anatomist, Professor Ludwig Fick, in whose department he began his career as Prosector, and to whom he owed facilities without which he could not have carried out his earlier researches; and in an even higher degree to the great chemist, R. W. Bunsen, from whom he derived that training in the exact sciences which was to be of such inestimable value to him afterwards.

There is reason, however, to believe, that, as so often happens, Ludwig's scientific progress was much more influenced by his contemporaries than by his seniors. In 1847, as we learn on the one hand from du Bois-Reymond, on the other from Ludwig himself, he visited Berlin for the first time. This visit was an important one both for himself and for the future of Science, for he there met three men of his own age, Helmholtz, du Bois-Reymond, and Brücke, who were destined to become his life-friends, all of whom attained to the highest distinction, and one of whom is still living. They all were full of the same enthusiasm. As Ludwig said when speaking of this visit: 'We four imagined that we should constitute Physiology on a chemico-physical foundation, and give it equal scientific rank with Physics; but the task turned out to be much more difficult than we anticipated.' These three young men, who were devoted disciples of the great

anatomist, had the advantage over their master in the better insight which their training had given them into the fundamental principles of scientific research. They had already gathered around themselves a so-called 'physical' school of Physiology, and welcomed Ludwig on his arrival from Marburg as one who had of his own initiative undertaken in his own University *das Befreiungswerk aus dem Vitalismus.*

The determination to refer all vital phenomena to their physical or chemical counterparts or analogues, which, as I have said, was the dominant motive in Ludwig's character, was combined with another quality of mind which, if not equally influential, was even more obviously displayed in his mode of thinking and working. His first aim, even before he sought for any explanation of a structure or of a process, was to possess himself, by all means of observation at his disposal, of a complete objective conception of all its relations. He regarded the faculty of vivid sensual realization (*lebendige sinnliche Anschauung*) as of special value to the investigator of natural phenomena, and did his best to cultivate it in those who worked with him in the laboratory. In himself, this *objective* tendency (if I may be permitted the use of a word which, if not correct, seems to express what I mean) might be regarded as almost a defect, for it made him indisposed to appreciate any sort of knowledge which deals with the abstract. He had a disinclination to philosophical speculation which almost amounted to aversion, and, perhaps for a similar reason, avoided the use of mathematical methods even in the discussion of scientific questions which admitted of being treated mathematically—contrasting in this respect with his friend du Bois-Reymond, resembling Brücke. But as a teacher the quality was of immense use to him. His power of vivid realization was the *substratum* of that many-sidedness which made him, irrespectively of his scientific attainments, so attractive a personality.

I am not sure that it can be generally stated that a keen scientific observer is able to appreciate the artistic aspects of Nature. In Ludwig's case, however, there is reason to think that the aesthetic faculty was as developed as the power of scientific insight. He was a skilful draughtsman, but not a musician; both arts were, however, a source of enjoyment to him. He was a regular frequenter of the *Gewandhaus* concerts, and it was his greatest pleasure to

bring together gifted musicians in his house, where he played the part of an intelligent and appreciative listener. Of painting he knew more than of music, and was a connoisseur whose opinion carried weight. It is related that he was so worried by what he considered bad art, that after the redecoration of the *Gewandhaus* concert-room, he was for some time deprived of his accustomed pleasure in listening to music.

Ludwig's social characteristics can only be touched on here in so far as they serve to make intelligible his wonderful influence as a teacher. Many of his pupils at Leipzig have referred to the *schöne Gemeinsamkeit* which characterized the life there. The harmonious relation which, as a rule, subsisted between men of different education and different nationalities, could not have been maintained had not Ludwig possessed, side by side with that inflexible earnestness which he showed in all matters of work or duty, a certain youthfulness of disposition which made it possible for men much younger than himself to accept his friendship. This sympathetic geniality was, however, not the only or even the chief reason why Ludwig's pupils were the better for having known him. There were not a few of them who for the first time in their lives came into personal relation with a man who was utterly free from selfish aims and vain ambitions, who was scrupulously conscientious in all that he said and did, who was what he seemed, and seemed what he was, and who had no other aim than the advancement of his science, and in that advancement saw no other end than the increase of human happiness. These qualities displayed themselves in Ludwig's daily active life in the laboratory, where he was to be found whenever work of special interest was going on; but still more when, as happened on Sunday mornings, he was 'at home' in the library of the institute—the corner room in which he ordinarily worked. Many of his 'scholars' have put on record their recollections of these occasions; the cordiality of the master's welcome, the wide range and varied interest of his conversation, and the ready appreciation with which he seized on anything that was new or original in the suggestions of those present. Few men live as he did, '*im Ganzen, Guten, Schönen*', and of those still fewer know how to communicate out of their fullness to others.

III. The Old and the New Vitalism.

Since the middle of the century the progress of Physiology has been continuous. Each year has had its record, and has brought with it new accessions to knowledge. In one respect the rate of progress was more rapid at first than it is now, for in an unexplored country discovery is relatively easy. In another sense it was slower, for there are now scores of investigators for every one that could be counted in 1840 or 1850. Until recently there has been throughout this period no tendency to revert to the old methods—no new departure—no divergence from the principles which Ludwig did so much to enforce and exemplify.

The wonderful revolution which the appearance of the *Origin of Species* produced in the other branch of Biology, promoted the progress of Physiology by the new interest which it gave to the study, not only of structure and development, but of all other vital phenomena. It did not, however, in any sensible degree affect our *method* or alter the direction in which Physiologists had been working for two decades. Its most obvious effect was to sever the two subjects from each other. To the Darwinian epoch comparative Anatomy and Physiology were united, but as the new Ontology grew it became evident that each had its own problems and its own methods of dealing with them.

The old vitalism of the first half of the century is easily explained. It was generally believed that, on the whole, things went on in the living body as they do outside of it; but when a difficulty arose in so explaining them the Physiologist was ready at once to call in the aid of a '*vital force*'. It must not, however, be forgotten that, as I have already indicated, there were great teachers (such, for example, as Sharpey and Allen Thomson in England, Magendie in France, Weber in Germany) who discarded all vitalistic theories, and concerned themselves only with the study of the time- and place-relations of phenomena; men who were before their time in insight, and were only hindered in their application of chemical and physical principles to the interpretation of the processes of life by the circumstance that chemical and physical knowledge was in itself too little

advanced. Comparison was impossible, for the standards were not forthcoming.

Vitalism in its original form gave way to the rapid advance of knowledge as to the correlation of the physical sciences, which took place in the forties. Of the many writers and thinkers who contributed to that result, J. R. Mayer and Helmholtz did so most directly, for the contribution of the former to the establishment of the Doctrine of the Conservation of Energy had physiological considerations for its point of departure: and Helmholtz, at the time he wrote the '*Erhaltung der Kraft*', was still a Physiologist. Consequently when Ludwig's celebrated *Lehrbuch* came out in 1852,—the book which gave the *coup de grâce* to vitalism in the old sense of the word.—his method of setting forth the relations of vital phenomena by comparison with their physical or chemical counterparts, and his assertion that it was the task of Physiology to make out their necessary dependence on elementary conditions, although in violent contrast with current doctrine, were in no way surprising to those who were acquainted with the then recent progress of research. Ludwig's teaching was indeed no more than a general application of principles which had already been applied in particular instances.

The proof of the non-existence of a special 'vital force' lies in the demonstration of the adequacy of the known sources of energy in the organism to account for the actual day-by-day expenditure of heat and work—in other words, on the possibility of setting forth an energy balance sheet, in which the quantity of food which enters the body in a given period (hour or day) is balanced by an exactly corresponding amount of heat produced or external work done. It is interesting to remember that the work necessary for preparing such a balance sheet (which Mayer had attempted but, from want of sufficient data, failed in) was begun thirty years ago in the laboratory of the Royal Institution by the present Foreign Secretary of the Royal Society. But the determinations made by Dr. Frankland related to one side of the balance sheet, that of income. By his researches in 1866 he gave Physiologists for the first time reliable information as to the heat value (i.e. the amount of heat yielded by the combustion) of different constituents of food. It still remained to apply methods of exact measurement to the expenditure side of the account. Helmholtz had estimated this, as regards man, as best he

might ; but the technical difficulties of measuring the expenditure of heat of the animal body appeared until lately to be almost insuperable. Now that it has been at last successfully accomplished, we have the experimental proof that in the process of life there is no production or disappearance of energy. It may be said that it was unnecessary to prove what no scientifically sane man doubted. There are, however, reasons why it is of importance to have objective evidence that food is the sole and adequate source of the energy which we day by day or hour by hour disengage, whether in the form of heat or external work.

In the opening paragraph of this section it was observed that *until recently* there had been no tendency to revive the vitalistic notion of two generations ago. In introducing the words in italics I referred to the existence at the present time in Germany of a sort of reaction, which under the term 'Neovitalismus' has attracted some attention—so much indeed that at the *Versammlung Deutscher Naturforscher* at Lübeck last September, it was the subject of one of the general addresses. The author of this address (Prof. Rindfleisch) was, I believe, the inventor of the word, but the origin of the movement is usually traced to a work on Physiological Chemistry which an excellent translation by the late Dr. Wooldridge has made familiar to English students. The author of this work owes it to the language he employs in the introduction on 'Mechanism and Vitalism', if his position has been misunderstood, for in that introduction he distinctly ranges himself on the vitalistic side. As, however, his vitalism is of such a kind as not to influence his method of dealing with actual problems, it is only in so far of consequence as it may affect the reader. For my own part I feel grateful to Professor Bunge for having produced an interesting and readable book on a dry subject, even though that interest may be partly due to the introduction into the discussion of a question which, as he presents it, is more speculative than scientific.

As regards other physiological writers to whom vitalistic tendencies have been attributed, it is to be observed that none of them have even suggested that the doctrine of a 'vital force' in its old sense should be revived. Their contention amounts to little more than this, that in certain recent instances improved methods of research appear to have shown that processes, at first regarded as entirely physical or chemical, do not conform so precisely as they

were expected to do to chemical and physical laws. As these instances are all essentially analogous, reference to one will serve to explain the bearing of the rest.

Those who have any acquaintance with the structure of the animal body will know that there exists in the higher animals, in addition to the system of veins by which the blood is brought back from all parts to the heart, another less considerable system of branched tubes, the lymphatics, by which, if one may so express it, the leakage of the blood-vessels is collected. Now, without inquiring into the *why* of this system, Ludwig and his pupils made and continued for many years elaborate investigations which were for long the chief sources of our knowledge, their general result being that the efficient cause of the movement of the lymph, like that of the blood, was mechanical. At the Berlin Congress in 1890 new observations by Professor Heidenhain of Breslau made it appear that under certain conditions the process of lymph formation does not go on in strict accordance with the physical laws by which leakage through membranes is regulated; the experimental results being of so unequivocal a kind that, even had they not been confirmed, they must have been received without hesitation. How is such a case as this to be met? The 'Neovitalists' answer promptly by reminding us that there are cells, i.e. living individuals, placed at the inlets of the system of drainage without which it would not work, that these let in less or more liquid according to circumstances, and that in doing so they act in obedience, not to physical laws, but to vital ones—to laws which are special to themselves.

Now, it is perfectly true that living cells, like working bees, are both the architects of the hive and the sources of its activity; but if we ask how honey is made, it is no answer to say that the bees make it. We do not require to be told that cells have to do with the making of lymph, as with every process in the animal organism; but what we want to know is *how* they work, and to this we shall never get an answer so long as we content ourselves with merely explaining one unknown thing by another. The action of cells must be explained, if at all, by the same method of comparison with physical or chemical analogues that we employ in the investigation of organs.

Since 1890 the problem of lymph formation has been attacked by a number of able workers—among others in London, by Dr. Starling of Guy's Hospital, who, by

sedulously studying the conditions under which the discrepancies between the actual and the expected have arisen, has succeeded in untying several knots. In reference to the whole subject, it is to be noticed that the process by which difficulties are brought into view is the same as that by which they are eliminated. It is one and the same method throughout, by which step by step, knowledge perfects itself—at one time by discovering errors, at another by correcting them ; and if at certain stages in this progress difficulties seem insuperable, we can gain nothing by calling in, even provisionally, the aid of any sort of *Eidolon*, whether 'cell', 'protoplasm', or internal principle.

It thus appears to be doubtful whether any of the biological writers who have recently professed vitalistic tendencies are in reality vitalists. The only exception that I know is to be found in the writings of a well-known worker, Hans Driesch[1], who has been led by his researches on what is now called the Mechanics of Evolution, to revert to the fundamental conception of vitalism, that the laws which govern vital processes are not physical, but biological —that is, peculiar to the living organism, and limited thereto in their operation. Driesch's researches as to the modifications which can be produced by mechanical interference in the early stages of the process of ontogenesis have enforced upon him considerations which he evidently regards as new, though they are familiar enough to Physiologists. He recognizes that although by the observation of the successive stages in the ontogenetic process, one may arrive at a perfect knowledge of the relation of these stages to each other, this leaves the efficient causes of the development unexplained (*führt nicht zu einem Erkenntniss ihrer bewirkenden Ursachen*)—it does not teach us why one form springs out of another. This brings him at once face to face with a momentous question. He has to encounter three possibilities—he may either join the camp of the biological agnostics and say with du Bois-Reymond, not only '*ignoramus*' but '*ignorabimus*' ; *or* be content to work on in the hope that the physical laws that underlie and explain organic Evolution may sooner or later be discovered; *or* he may seek for some hitherto hidden Law of Organism, of which the known facts of Ontogenesis are the expression,

[1] Driesch, 'Entwicklungsmechanische Studien': a Series of ten Papers, of which the first six have appeared in the *Zeitsch. f. w. Zoologie*, vols. liii and lv, the rest in the *Mittheilungen* of the Naples Station.

and which, if accepted as a Law of Nature, would explain everything. Of the three alternatives Driesch prefers the last, which is equivalent to declaring himself an out-and-out vitalist. He trusts by means of his experimental investigations of the Mechanics of Evolution to arrive at 'elementary conceptions' on which by 'mathematical deduction'[1] a complete theory of Evolution may be founded.

If this anticipation could be realized, if we could mentally construct with the aid of these new *Principia* the ontogeny of a single living being, the question whether such a result was or was not inconsistent with the uniformity of Nature, would sink into insignificance as compared with the splendour of such a discovery.

But will such a discovery ever be made? It seems to me even more improbable than that of a physical theory of organic evolution. In the meantime it is satisfactory to reflect that the opinion we may be led to entertain on this theoretical question need not affect our estimate of the value of Driesch's fruitful experimental researches.

[1] 'Elementarvorstellungen . . . . die zwar mathematische Deduktion aller Erscheinungen aus sich gestatten möchten.' Driesch. 'Beiträge zur theoretischen Morphologie.' *Biol. Centralblatt*, vol. xii, p. 539, 1892.

THE CELLULAR PATHOLOGY OF TO-DAY[1]

Gentlemen,—May I be permitted first of all to express to you my high appreciation of the honour you have conferred on me by inviting me to address you on so important an occasion. I desire at the same time to thank our French colleagues on my own behalf and on that of the English members of the Congress for their friendly welcome.

Forty-seven years ago, after graduating in Edinburgh, I repaired to Paris for the purpose of completing my medical education. At that time Paris afforded facilities for clinical study which were not to be had in England. To myself it was an additional inducement that there were in Paris well-organized laboratories for practical instruction in chemistry, histology, and physiology. Moreover, the period was one of great activity and progress. Claude Bernard had recently achieved his greatest discoveries, those relating to the functions of the liver, the pancreas, and the processes of digestion. Brown-Séquard had just demonstrated the functions of the vaso-motor nerves and was already engaged in his investigations of the functions of the spinal cord. It was to Paris that every student repaired who desired to become acquainted with the most recent advances in physiology.[2]

The progress of pathology at the time we are considering was no less remarkable than that of physiology. It was then that the 'cellular pathology'—the doctrine which has so powerfully influenced the progress of scientific medicine—was originated by Virchow. Nearly twenty years had elapsed since the discoveries of Schwann and the promulgation of the 'doctrine of the cell'. The science of

[1] Address to the Thirteenth International Medical Congress, Paris, 1900.

[2] In 1852 3 the number of English students in Paris was considerable. I worked in the laboratory of the distinguished chemist Gerhardt and subsequently in that of Wurtz and Verdeil, at the same time following the demonstrations of Bernard. The day was divided between hospital attendance before, and laboratory work after, breakfast.

histology had been developed by Charles Robin in France, by Bowman in England, by Henle and above all by Kölliker in Germany. Of these remarkable men one only is still with us. To Kölliker we can to-day address our congratulations on a life-work of unparalleled fruitfulness. It was from Kölliker's university, Würzburg, that the 'cellular pathology' emanated. The book which bore this title was not published until Virchow had migrated to Berlin (in 1856), but since 1847, when the first volume of the *Archiv* appeared, the doctrine had been step by step set forth. A few years later (1862) von Recklinghausen made the capital discovery that the colourless corpuscle of the blood was not, as had been previously thought, a vesicle containing a nucleus, but rather an organism full of life and movement which possessed the wonderful faculty of incorporating minute particles suspended in its liquid environment. Five years later (1867) another pupil of Virchow, Cohnheim, made the further discovery that pus can be formed by the emigration from the blood-vessels of these same corpuscles, but it was not until after Cohnheim's death that the key to the mystery which he had in vain sought to penetrate was discovered by Leber[1] in the analogy of the phenomenon in question to that to which the botanist Pfeffer had given the name of 'chemotropism'.

I have adverted to these discoveries relating to the functions of the cell for the purpose of setting forth the relations between the cellular pathology of to-day and the cellular pathology as originated by Virchow in the 'fifties'. There can be no doubt that the tendency of pathological research has undergone a marked change since that time. Formerly the histological characters of morbid processes—of inflammation, of tuberculosis, and of other organic diseases—were the subjects of special interest. Now it is the microbe. It would, however, be a mistake to suppose that the new science of microbiology has hindered the progress of histology. On the contrary, histological research has never been pursued with more zeal and success than during what may be called the bacteriological period (1880 to the present time). I need only refer to the discovery of karyokinesis (1873–82) and to the wonderful advances in the microscopical study of the nervous system which we owe to Golgi, to Cajal, and to the army of artists in histology who are now working in the same field.

[1] Leber, *Die Entstehung der Entzündung*, &c., Leipzig, 1891.

There are at the present moment few who doubt the value of bacteriological diagnosis or of serum therapeutics. These are the practical, life-saving results of bacteriological research. But it is not in these directions that microbiology is at present exercising its chief influence on the science of medicine ; it is rather that it has given us a new and more comprehensive idea of the physiological functions of the cell and consequently of cellular pathology. Pathology is no less fundamentally cellular than it was when Virchow so designated it in the title of his book. But the word 'cellular' has now a wider signification, for it comprises not merely histological changes, but the chemical reactions which subsist between the cell and its environment. Formerly we regarded each kind of cell as having a single special function proper to itself, but the progress of investigation has taught us that each species of cell possesses a great variety of chemical functions and that it may act on the medium which it inhabits and be acted upon by it in a variety of ways. Thus, for example, we think of the colourless corpuscles of the blood (or, as we now call them, leucocytes) not merely as agents in the process of suppuration or as typical examples of contractile protoplasm, but rather as living structures possessing chemical functions indispensable to the life of the organism. Similarly we have come to regard the blood disc, which formerly we thought of merely as a carrier of haemoglobin, as a living cell possessed of chemical susceptibilities which render it the most delicate reagent we can employ for the detection of abnormal conditions in the blood.

The tendency of recent research is to show that the reactions to which I have referred as chemical functions of the cell (action of the cell on its environment—action of the environment on the cell) are the work of ferments—intrinsic or extrinsic—which are products of the evolution of the living cell and therefore to which we may apply the term 'enzymes'[1] devised for them by the great physiologist whose loss we at this moment so bitterly deplore. Can we go a step further and accept the suggestion which presents itself that *all* the functions of the

[1] The word 'enzyme' was introduced by Kühne in 1874. It has been adopted as a general term for soluble ferments which originate from cells by Professor J. R. Green in his important work on Fermentation. For this purpose it is much to be preferred to the word 'diastase', which is used by Duclaux and other French writers in the same general sense.

cell are due to the agency of enzymes? The question is one which we are bound to answer with great reserve. The utmost that can be said is, that if we exclude from consideration the functions which Bichat designated as functions of animal life—in other words, the functions proper to the nervous system and under its immediate control—we may as regards organic functions accept the suggestion as probably true. Recent researches have plainly indicated that in the case of the disease-producing micro-organisms, the specific functions which for years we regarded as proper to, and inseparable from the cell, belong essentially to the enzymes which they contain. It has been further shown that similar statements can be made as regards ferment-processes which differ widely from each other and no less widely from those induced by bacteria. Thus, if I may be allowed to quote the striking language employed by M. Duclaux[1]: 'There are ferments which by slipping a molecule of water into the midst of a complex molecule divide it into two or three more simple ones, as an iron wedge splits a block of wood, and others which can reconstitute the block by reuniting its dissociated elements. Again, there are enzymes which conduct oxygen to the bodies on which they exert their action', and others 'which break up a chemical compound as the explosion of a shell shatters a wall.' So that in the domain of microbiology the enzyme may in a certain sense be said to have 'dethroned the cell.' For if, as M. Duclaux continues, we can extract from the cell a substance which breathes for it, another which digests for it, another which elaborates the simple from the complex, and finally another which reconstitutes the complex from the simple, the cell can no longer be considered as *one*, but rather as a complicated machine, the working of which is for the most part dependent on enzymes, which, however numerous and varied may be the processes in which they are engaged, all follow and obey the universal law of adaptation and all contribute to the welfare and protection of the organism.

Admitting that we may provisionally delegate the function of the cell to its enzymes the question may still be asked whether by doing so we acquire a better understanding of

[1] M. Duclaux is the accomplished Director of the Pasteur Institute. The passage quoted is in the final chapter of the second volume of his comprehensive treatise on *Microbiology*.

organic life. The answer may, I think, be easily given. The only way in which we can explain a vital phenomenon is by determining its non-vital conditions. The essence of physiological experimentation consists in the elimination of the mystery of life.[1] As regards each chemical function of the cell, taken separately, the first step towards this result is to reproduce *in vitro* what one has observed *in vivo*. Even though it may be impossible to state the nature of the process in chemical language, it is a great advantage to be able to study its relation to external conditions which have nothing to do with its vital origin. In illustration of this we may take a physiological process in relation to the nature of which the vitalistic idea has held its ground with great pertinacity—the alcoholic fermentation. It was not until seventy years had elapsed since the discovery of the yeast-plant, that Buchner, by employing more effectual methods than had been devised by his predecessors, was at last able to prove that the alcohol-producing enzyme can be separated from the zymogenetic cell; i.e. that the chemical process which has been the source of so much good and evil to the human race may be brought about without the direct intervention of the living protoplasm. Another instance of nearly equal interest is that of the recent discovery and investigation *in vitro* by M. G. Bertrand[2] of the ferments on which certain plant-cells depend for the power which they possess of quasi-respiratory oxidation and the experimental proof which he has given that this is effected by the agency of an oxidizing enzyme. It cannot be said that in either of these instances the way in which the enzyme acts can be *explained*.[3] In each case a process hitherto regarded as vital has been shown to be chemical, but the mode of action of the enzyme cannot as yet be stated in chemical language, so that the value of the dis-

[1] That is, so much of the mystery as depends on vital conditions. Many physical and chemical processes—e.g. the production of muscular force, and, as will be seen further on, the assimilation of carbon by plants—remain mysterious even after the vital element has been eliminated. The physiologist, however, has done his part and can hand over the unsolved problem to the chemist or physicist, as the case may be, with the assurance that eventually the explanation will be got at.

[2] *Comptes Rendus*, vols. cxxi and cxxiii, 1895–6.

[3] In physiology and pathology we use the words 'explained' and 'understand' in a much stricter sense than that in which they are used by some popular writers on biology. To prove that what happens is in accordance with the general law of adaptation is not to explain it.

covery lies in the biological importance of the process and the new power which the physiologist has gained of subjecting it to exact investigation. In furtherance of that investigation all that chemistry can do at present is to furnish us with efficient technical methods, but eventually—in the course of the next century—there can be no doubt that the purely chemical problems relating to the action of ferments which in the course of the next few years the biologist will one after another submit to the chemist will receive their answers. In the meantime let us avoid skating on thin ice. The value of the generalization that each chemical function of the cell is the work of an enzyme, however admissible it may seem, depends on the number and importance of the phenomena to the elucidation of which it has been successfully applied. So long, for example, as a process of such fundamental importance as that by which the plant cell with the aid of sunlight is capable of elaborating carbohydrate out of material derived from the atmosphere has not been shown to be zymotic, we must carefully guard against giving the theory anything like the authority of a biological law.

May I now, by way of sequel to the general considerations relating to the chemical functions of cells which have occupied our attention, say something of their bearing on what may be called the 'cellular pathology' of to-day, the relation of which to that of forty years ago I have already indicated. The examples to which I referred just now related to the physiology of the plant cell. Our pathological problems relate to the constituent cells of our own bodies, the chemical functions of which present much greater difficulties to the investigator. It is not difficult to understand why more progress has been made in the investigation of isolated cells, such as coloured and colourless blood corpuscles and the nucleated cells which constitute the adenoid tissues, than in those relating to the constituent cells of organs. From the study of the cells of adenoid tissues we have learned that the nucleus not only governs the development of the cell but that it has chemical endowments which distinguish it from the protoplasm which surrounds it. I need only mention in this connexion the researches made by my friend Sir L. Brunton more than thirty years ago in Professor Kühne's laboratory, the subsequent work of Miescher the discoverer of nucleine, and the much more extended investigations of Professor

Kossel of Marburg, which have given us such exact information as to the products of decomposition of nuclear substance.

As regards the coloured corpuscles, important discoveries have been made during the last few years in two directions. They have been studied first in relation to their physical properties, and particularly in relation to their osmotic relations to organic and inorganic substances in solution. I need only remind you of the work done by Professor Hamburger of Utrecht and by Dr. Hedin of Lund in Sweden. Still more interesting results have been obtained by investigations conducted in France and Germany relating to the physiological and chemical conditions which, on the one hand, render them capable of resisting the 'globulolytic' action of the blood plasma, or on the other hand, compel them to yield to that action.

As the subject of haemolysis at the present moment is receiving much attention, both here and elsewhere, I may be permitted to devote a few moments to its discussion. The fact that the blood disks are in general destroyed when they are introduced into the circulation of an animal of a different species has been long familiar to us. A few years ago it was demonstrated by Buchner that the haemolytic power has its seat in the plasma and consequently in the serum. Still later, M. Bordet of the Pasteur Institute made a remarkable discovery that the haemolytic process now in question is identical with the well-known 'bacteriolysis' which takes place when pathogenic micro-organisms are acted on by the serum of an immunized animal. By a numerous series of observations *in vitro* and *in vivo* he showed that the lytic substance in the serum which acts on the vibrio of cholera and that which acts on the blood-disks are identical, provided that the serum also contains a *tertium quid*—a certain substance which has a specific antagonistic relation to the *materies morbi* in the one case, to the 'alien' blood-disk in the other. In other words the intruding blood-disk acts, and is acted upon, exactly as if it were an infective agent. To this specific body M. Bordet gave the name of '*sensibiliser*' because it renders the blood-disk or the specific micro-organism susceptible to the attack of the normally present haemolytic ferment. The '*sensibiliser*' serves, so to speak, as a key, by the application of which the ferment is enabled to

penetrate into the interior of the blood-disk so as to act on its susceptible constituents. And inasmuch as it appears to be proved that every blood-disk is liable to be affected by the presence of a plurality of substances possessed of the specific *intromittent* function, it seems as if one must admit that it must possess a corresponding number of specific susceptibilities; for if there are several keys, each differing from all the rest, there must be as many locks to which they severally fit.[1]

It is apparent that these beautiful researches have a special interest in relation to the occurrence of haemolysis as a symptom, i.e. to the solution of the blood-disks which occurs in a variety of morbid states; but it is by the help which they are likely to afford us in our efforts to understand the origin and essence of disorders of nutrition, that the facts which have been disclosed relating to the chemical physiology of the blood-cells are most likely to be of service. In the action of the liquor sanguinis on alien blood-disks we have an exemplification of the way in which ferments may play a part as the instruments by which the constituent cells of different organs may influence each other for good or evil without coming into contact. When we turn from the blood-disks and leucocytes to the constituent cells of organs, it is obvious that the difficulties of investigation are infinitely greater. The suggestion which naturally presents itself is that the actions on each other of the constituent cells of different organs within the body of the same individual, are brought about in a similar way to those which we have learnt to recognize in the actions of indigenous blood-corpuscles on aliens; that is to say, that among the metabolic products which each species of cell throws into the blood-stream or lymph-stream there are specific substances which are analogous to M. Bordet's '*sensibilisers*' in so far that by their presence or absence the prosperity or decadence—the functional activity or the contrary—of other cells is

[1] Those who were present at the lecture given by Professor Ehrlich at the Royal Society last spring will notice that Bordet's 'haemolytic substance', or alexine, corresponds to Ehrlich's 'complement', and Bordet's '*sensibiliser*' to Ehrlich's 'immune body'. The language differs, but the experimental data are in agreement. It is proved that two substances are concerned, viz. a haemolytic substance or 'alexine' normally contained in the liquor sanguinis, which cannot, however, exercise its haemolytic action unless the key to the lock—the *sensibiliser*—be present. M. Bordet's principal results were published in 1898. A second paper appeared this year.

directly affected. Vague as such language appears, it may serve to indicate the lines along which we must work in our efforts to solve some of the most important problems of cell-pathology. In all dystrophic conditions, e.g. in the functional disorder of muscle and of liver-cell which renders these structures incapable of holding carbohydrate in reserve—in the autotoxy which manifests itself in the graver form of diabetes—in the 'acid intoxication' which is associated with acute disintegration of the liver—in morbid conditions attributable to 'internal secretions'—in all of these instances we have to do with disorders of the chemical functions of cells. In all our endeavours to ascertain the immediate and determining causes of these disorders we may with no less advantage at the present time than formerly allow ourselves to be guided by the principles and methods of the cellular pathology as Virchow taught them nearly half a century ago. The principles remain unchanged; as regards the methods, we have only to add to those we then learned the newer ones which the progress of knowledge has placed within our reach.

OUR DUTY TO THE CONSUMPTIVE BREAD-EARNER [1]

Towards the end of next month, as you all know, a Congress will be held in London 'for the prevention of tuberculosis'. It will, without doubt, receive the cordial support of the medical profession, but this support cannot be given effectively unless we have beforehand some more precise notion than is expressed in the word 'prevention' of the purposes for the accomplishment of which we are invited to meet. We may, I trust, take it for granted that the aim of the Congress will be practical rather than scientific, that we shall meet rather to act than to discuss pathological or even etiological questions. The researches of the past twenty years have taught us much that we did not know before both as to the nature and causes of tuberculous diseases, and have thereby furnished us with generally accepted principles for our guidance. What now chiefly concerns us is to come to right conclusions as to the way in which these principles ought to be applied.

Preventive and General Sanitary Measures.

These practical considerations may be divided according as they concern on the one hand the individual invalid whom we desire to benefit, or on the other the community which we desire to protect. Among the measures to be used for the former purpose we may further distinguish between those which aim at the relief of the patient's symptoms by medical treatment, and those which tend to the re-establishment of his health and working power. It is to this last subject (as I trust the title of my address will have indicated) that I wish specially to invite your attention; but before I go further I must, to avoid misunderstanding, say a word as to prevention.

The promoters of the great movement which culminated in the Berlin Congress of 1899, *Zur Bekämpfung*

[1] Address to the Oxford and District and Reading and Upper Thames Branches of the British Medical Association, 1901.

der Tuberculose (How to Conduct the War against Tuberculosis), worked along the two parallel lines I have just referred to—the preventive and the sanatory. That movement originated, I need not say, in Germany; but let us not forget that the admirable work which has been recently done in that country relating to the etiology of tubercle was founded on the inquiries made some forty years ago under Sir John Simon's direction by the late Sir George Buchanan and Dr. Headlam Greenhow. It was they and their chief that first taught us to regard phthisis as a preventable disease. The influence of the knowledge thus gained was no doubt enhanced by its practical exemplification in the diminution of the mortality from phthisis which resulted from sanitary improvement; for the observation of what had been thus accomplished many years before in England certainly exercised a considerable influence in bringing about the movement in Germany to which I have referred.

As to the practical carrying out of preventive measures against phthisis there is happily little difference of opinion. No one, so far as I know, doubts that the overcrowded, sordid dwellings that the poorer class of working people are compelled to occupy contain in themselves sources of danger, nor does any one question the disease-producing influence of poverty. In like manner, the researches conducted by Dr. Martin for the first Tuberculosis Commission have taught us what we chiefly required to know as to the dangers of meat and milk; and those of Professor Cornet in Berlin have informed us as to the mechanism and sources of pulmonary infection. In these various ways the data for a new prophylactic code have been furnished, as to the effectiveness of which there is little room for discussion.

Our Duty as Citizens and as Christians to the Consumptive Toiler.

As I said just now, there are two parallel lines of action along which the combat with pulmonary tubercle must be carried on. Let me now ask you, leaving questions of prophylaxis for the moment out of consideration, to fix your attention on the vast population of persons who gain their bread by labour, and to bear in mind that of these a large proportion do not earn enough to maintain themselves and

their families in anything like comfort. They may not be destitute and are not objects of charity, but the conditions under which they live are so unfavourable as to render them more liable than the well-to-do classes to the invasion of the tuberculous infection. When such a person becomes phthisical, he loses the one possession which constitutes his fortune. He loses his earning power. As the disease progresses the burden of poverty becomes harder and harder to bear. He suffers himself, and those who are dependent on him for their subsistence suffer with him. Their condition is helpless, and, unless more effectual means of aiding him than are at present available can be devised, hopeless. No one doubts that we are bound in common Christian charity to do what we can for those whom sickness has brought by no fault of their own into destitution, but most people find it much more difficult to understand that the earning power of the workman is a commodity far too valuable to be wasted, and that the duty of preserving it is no less incumbent on us as citizens than as Christians. The question I desire to place before you relates to both of these obligations, but chiefly to the second. I would ask you for the moment to consider the case of the workman in the first stage of consumption with exclusive reference to his loss of earning power. Let us assume that it is only half of what it was a year earlier and that in still greater proportion his value to the community has diminished. How can we help him to guard against its falling to a lower level? How can we help him so to husband the strength that remains to him, that life may be still worth living—not a mere slow descent into the dark valley?

The Kind of Help the Consumptive Bread-earner requires.

The answer to the question, How is this to be done? depends on what we know of the character of the evil we have to combat. And here I must ask your permission for a moment to speak as if I were addressing laymen. We call the disease consumption—phthisis—because it gradually but surely deprives its victim of health and strength, but it is not less characteristic of it that the decline is not a continuous one. We habitually divide it into stages—an initial stage characterized by gradual

impairment of all functions, and, as regards the affected organ, by consolidation; and a second stage, by fever, rapid emaciation, cough, and expectoration, and the 'physical signs' of disintegration. But although it is convenient to call them stages, the two conditions are not consecutive. The first stage is not an even downward progress, but a progress broken by catarrhal and febrile accessions, each of which is attended by signs of disintegration. During these attacks the patient is unfit for work and requires bodily rest in bed. In general, the tendency of the attack is to pass off. If the patient is taken care of he returns after a few weeks' illness to the same general condition that he was in before, but a little weaker, a little more incapable. Finally comes the time when the process of breaking down goes on more rapidly, the tendency to a fatal termination of the disease is more decided, when, instead of moderate health interrupted by occasional illness, you have illness with occasional intervals of partial recovery.

I have referred to these familiar facts in order to make it clear that the kind of help to be given must be determined by the condition of the individual that we desire to benefit. Our main purpose is *to enable the bread-earner whose health has been impaired by disease to make the best of the strength that remains to him.*

With this view two things are necessary. The first is to bring within his reach havens of refuge, to which he can betake himself with the assurance that he will find an immediate welcome during the periods of temporary illness to which he is liable. The second is to furnish him with the means of relieving himself from the burden of overwork from the moment that the progress of the disease makes him incapable of sustaining it.

Importance of Prompt Admission to Hospital whenever the State of the Patient requires it.

Of these two requirements the first can be dealt with much more easily than the second. Wherever there is actual suffering, Christian charity and pity are at hand to alleviate it. The enormous sums which are annually contributed by the charitable for the relief of consumptive patients might, if rightly used, almost suffice for the

purpose. Unfortunately, the same vicious principle which prevails in the administration of other medical charities supported by voluntary contributions is all-powerful in our hospitals for consumption. Benefactors are not satisfied with the assurance that their gifts are administered by competent persons in the most effectual way for securing the end which they profess to have in view. They are unwilling to forgo their undeniable right to do what they will with their own. Like those charitable donors of whom we read in Holy Scripture, they must needs 'keep back part of the price'—not indeed in the form of money, but in the exercise of a paltry kind of patronage.

What is required is roughly that there should be a haven of refuge in every industrial centre (consisting of a hospital ward or a hospital) to which the need of the sufferer—in this case the existence of the physical signs and symptoms of advancing phthisis—should be the only claim for admission, and that every admission should be conditional on liability to removal at the discretion of the medical authority in charge. By the enforcement of this condition the risk of converting the temporary refuge into a permanent asylum would be guarded against, while at the same time the workman who had found the tide of adversity too strong for him would be enabled to regain his footing, and once more resume the struggle, if not with renewed vigour, at least with some renewal of hope and courage.

Now it cannot be said that anything effectual has as yet been done in the direction I have indicated. In London the phthisical workman has no great difficulty in obtaining an out-patient letter for a general or special hospital; but from the moment that the progress of his disease renders him unable to attend, his position becomes a painful one. The nature of his disease shuts against him the doors of the general hospital, and if by good luck he obtains an 'in-patient letter' for a special hospital, the delay which undue regard for the rights of governors necessarily entails is so long that he does not arrive at his desired haven until too late. Prompt admission might have restored him to comparative health, but the time for successful treatment has been spent under the unfavourable conditions of his home, where he has not only undergone suffering that might have been prevented, but has been a source of danger to his neighbours. The risk of allowing a patient with acute symptoms to live among others is

indeed, irrespectively of the detriment to himself, a sufficient reason for his immediate removal from his surroundings.

Measures to be taken for the Prolongation of the Life of the Consumptive Bread-earner and for the Maintenance of his Earning Power.

Let us now go on to the second of the two requirements to which I referred a few minutes ago. The obligation to make adequate provision for the prompt medical relief of the phthisical workman during the acute accessions to which he is liable is one which no one will be inclined to disregard, but it is only a small part of the duty that is imposed upon us. The motive which actuates us should be rather economic than charitable. Our aim should be not so much the relief of suffering as the maintenance of earning power. The important problem we have in hand is to determine the line of action which ought to be adopted to help those who, although they are in a certain true sense invalids, neither desire nor require the intervention of charity.

For this end the first step to be taken is to obtain information as to the persons whom we desire to benefit. This information is of value as a guide in carrying out sanitary improvements, for we know that the prevalence of phthisis is now recognized as one of the most certain indications of sanitary defects. It is not, however, for purposes of sanitary administration that it is chiefly required, but as enabling us to enter into personal relation with the workman in the initial stage of the disease, this being the first step towards giving him the aid that he needs. To a certain extent the investigations of the prevalence and distribution of phthisis which have been made at Brighton, in Oxford, and a few other places by zealous medical officers of health have, so far as these places are concerned, accomplished this end. The results have at all events been sufficiently good to show that if the system of voluntary registration could be extended all over the country it would afford an excellent foundation for prophylactic measures; but *sanitary improvement is only one of the lines along which we have to work in the combat with pulmonary tuberculosis.* The knowledge that we gain as

to the prevalence of phthisis, whether obtained by registration or otherwise, will be of little use to the man who has lost his working power, unless we are in a position to aid him in regaining it. Now there is no difficulty in indicating by what means this must be accomplished.

To the phthisical invalid who without requiring much doctoring or nursing finds his strength gone and his earning capacity reduced to half of what it was before, the one restorative that is indispensable is temporary immunity from labour. To this the other two items of sanitary treatment, namely, good air and good food, are consequential. If we were now concerned with well-to-do persons, whether bread-earners or not, we should have to consider whether the home is not in many cases the best resting-place, but no such question can for a moment be entertained in dealing with the invalid workman. Consequently for him, if he is to have rest, good air, and good food, a resting-place away from his home must be provided. In other words, sanatoria for the poor are a necessity.

Origin of the Sanatorium Movement in Germany

It is ten years since this great necessity was recognized in Germany. It has not even yet been recognized in England. There are several reasons for this, but the chief one is that there are difficulties in establishing such sanatoria, which do not exist in any other European country. Why can we not do what has been successfully done elsewhere? I shall, I think, best answer this by giving you a sketch of the origin and progress of what may be called the sanatorium movement in Germany.

The initiation of the movement was somewhat as follows: The experience of sanatoria for persons in good circumstances had shown that in cases of consumption in the first stage judiciously selected, health could be maintained and life prolonged by placing these persons under favourable climatic influences combined with good food and moderate exercise. But the result would hardly have been brought about had not Dr. von Leyden, who has for years occupied a most prominent position as a physician in Berlin, given to the movement the required impulse by certain addresses delivered to the medical societies of Berlin in 1888 and 1889, in which he treated of the prevalence of consumption

among the working population as a matter of national concern, and insisted on the necessity of establishing sanatoria for the poor as the only means by which it could be combated. The result of these discussions, and more particularly of that which took place at the International Congress in 1890, was that von Leyden's suggestions were warmly taken up by leading men in and out of the profession, who at once set to work to devise practical ways of carrying the principle of sanatoria for the poor into effect. At the present moment sanatoria of this kind for men and women exist throughout the German Empire. Their number is still far below the requirement, but every year new institutions are added to the list. The success with which the work has been carried out can be best judged of by the following figures:

At the date of the last International Medical Congress (August, 1900) there were in the German Empire forty-nine sanatoria for the poor, of which the plans of forty-one were exhibited in the Department of Hygiene of the Great Exhibition. There were at that time eleven others in course of construction [1] (besides twenty-eight projected), so that there are probably about sixty of these institutions in operation at the present moment. The forty-nine sanatoria which were open a year ago contained 4,000 beds. The cost of their erection amounted to about £1,000,000, so that the cost per bed, including all internal fittings, may be estimated at £250. It is, however, held that in future this initial cost will not exceed £200 per bed.

The yearly expenditure for each occupied bed is estimated at £65, but about a fifth of this sum is not expended within the walls of the sanatorium, but goes to the maintenance when necessary of the families of the occupants during their period of residence. From this it follows that the total annual expenditure for maintaining the sanatoria was a year ago about a quarter of a million. It is anticipated that when the system is complete its maintenance will cost about five times as much—from a million to a million and a-half annually—that is, that this enormous sum will be required to bring within the reach of every working man and woman in Germany who is threatened with phthisis the means of doing the best that can be done for the maintenance of his earning capacity.

[1] Engelmann, Die deutschen Lungenheilstätten auf der Weltausstellung, *Zeitschrift für Tuberculose*, &c., vol. i, p. 217.

Compulsory Insurance.

As I have already indicated, the reason why we in England have not followed the example which we have had before us for so many years is that in Germany facilities exist for organizing a sanatorial system, which we are deprived of. The nature of these facilities I must now explain. The questions which present themselves *in limine* are : First, where does the money come from ? and secondly, How is the system brought into relation with the right people ? The first question may be answered at once by saying that the resources of the sanatoria are derived partly from organized charity, but chiefly from contributions of the workmen themselves—that, in short, the whole explanation of the success is expressed by the two words, *compulsory insurance*.[1] In Germany every workman whose income is less than £100 a year is required by a law which came into full operation about ten years ago to insure himself against the two greatest ills that flesh is heir to—sickness and old age.

In this way a national fund is brought into existence, which is applicable to any purpose directly beneficial to the contributors, whether it be the therapeutical or the sanatory treatment of the bread-earner himself or the maintenance of relatives dependent on him for their subsistence during the period of his incapacity for work. The number of contributors is about thirteen millions, of whom not much more than one per cent. are at any one time on the list of recipients as incapacitated for work by illness. Among these one in every three is phthisical. It is estimated that of the initial cost of the sanatoria for the poor which were in operation at the date of the International Medical Congress last autumn, half was furnished by the insurance funds, and that of the annual expenditure about three-quarters were derived from the same source. The remaining half-million for initial expenditure, and £60,000 for maintenance, were contributed by private liberality, by charitable organizations, or, in some cases, by municipal or other local authorities.

[1] For information as to the system of compulsory insurance see Düttmann and Gebhard's *Handausgabe des Invalidenversicherungsgesetzes*, Altenburg, 1900.

Selection of Suitable Persons for Treatment in Sanatoria.

As regards the second question, that of the selection of suitable cases for sanatory treatment, the facilities which compulsory—that is, universal—insurance offers for obtaining exact information as to the incidence of phthisis among the working population are obvious. By means of it the poorer bread-earners are brought together into a well-defined class within the limits of which every person who is disabled by phthisis, provided that his disease is not too advanced, has offered to him at the moment that he requires it the advantage of temporary rest in a sanatorium, with the assurance that he will not be turned out after a certain number of weeks, in obedience to a senseless rule of the institution, but will remain as long as in the judgement of the medical superintendent it is for his advantage to do so, whether it is for a few weeks or as many months. In this way the cases admitted into the sanatoria are to a certain extent *selected*—it is, at all events, possible to exclude those which are obviously unlikely to derive benefit.

This, however, is not the only or the chief use to which the data furnished by national insurance can be put. For our present purpose the information which it is capable of yielding as to the degree in which those who are admitted to sanatoria are benefited is still more important.

Relation between Poverty and Prevalence of Phthisis.

Before referring to these data let us for a moment consider the kind of people with whom we are concerned and the circumstances which affect their liability to disease. They are for the most part men in the prime of life, at an age when pulmonary tuberculosis is shown by insurance statistics to contribute no less than half of the total percentage of disability. Of the various external conditions which account for this liability poverty is by far the most influential. For if a comparison be made of persons who earn £1 a week (more or less) with those who earn from £2 to £3 a week, the annual mortality from phthisis is 4 per 1,000 of the former class as against 2 per 1,000 of the latter. It

is clear, therefore, that the workmen on whom in Germany the obligation to insure themselves against disease and old age is imposed by law, constitute a class in which the liability to pulmonary tuberculosis is greater than in any other. And inasmuch as these individuals do not all avail themselves of the sanatoria, it is possible to compare those who are so treated with those who are not.

Beneficial Influence of Residence in Sanatoria.

It is by a comparison of this kind, in which the life-history of persons treated in sanatoria is contrasted with that of persons of the same class who remain at home, that it can be best ascertained whether or not substantial benefit accrues to the community or to the individual from the sanatorial system. That system has not been in operation for a sufficient time for it to be possible to follow out the final result of sanatorial treatment. There are, however, facts which are sufficient to show the contrast between the two classes. In persons who, while receiving support when disabled by illness, are not removed from the unfavourable influences to which they are exposed in their homes, the duration of life after the beginning of illness is very much shorter than we are accustomed to find it among patients in easy circumstances. Among the latter, according to the very reliable estimate of Dr. C. T. Williams, the expectation of life is about seven and a half years; whereas of every 100 workmen in the initial stage of phthisis earning a little more or less than £1 a week, fifty will have died in three years. Had these 100 persons been treated sanatorially, statistics seem to me to show clearly that in about seventy per cent. health would have been so far maintained or restored, that they would have been able to support themselves and their families, and that if their condition had been investigated two years later, that is, three years after the first indications of illness, there would have been some fifty persons still capable of earning their daily bread. Of the remainder, some would no doubt have died, others would be in various degrees of illness or incapacity, but a large proportion would have been able to do partial days-works. This result does not at first sight seem very brilliant. If it were so, we might be sure that it was untrue. But the benefit conferred is nevertheless real and substantial. No delusive hope of cure is held out to the consumptive toiler,

but a new lease of life, though a short one, has been vouchsafed to him. A certain amount of earning power, which would otherwise have been lost, has been restored to him, and through him to the community of which he is a member. As regards the individual, it may seem of little importance whether the struggle is long or short, whether he dies now or two or three years hence, whether he is wholly incapacitated or able to earn a certain proportion only of what he earned before ; but if any means can be devised whereby a respite of even a few years may be secured to our tens of thousands of phthisical workmen, we are bound as Christians and as citizens to use them.

How can Sanatoria be supplied in England?

And now I come to my last point—to the most difficult question of all. Granted that sanatoria for the poor are a national necessity, how can this need be supplied in England?

Many will, I know, be inclined to shelve the question, considering it scarcely worth while to prolong a struggle which in each case must eventually and at no distant period end in death, or that no practical result can come of discussing it ; but those who admit that human life is of more value than anything else in the world, and that we have to gather up the fragments that remain even of consumptive life, that nothing be lost, will join with me in considering calmly and without prejudice whether there is any way by which the consumptive toiler can be effectually helped to keep hold of that remainder.

I do not recommend that we should imitate what has been done in Germany, but that we should ascertain what have been the essential conditions of success, and what are the reasons why they should not be realized in this country. These essential conditions relate :

1. To the choice of suitable sites and the erection and maintenance of suitable buildings.

2. To the selection of suitable cases, and to their prompt admission.

3. To the regulation under medical supervision of the time of residence of each invalid ; and

4. To the medical supervision of invalids after their discharge from the sanatorium.

It is obvious that the carrying out of these indications

all over England could not fail to require an enormous initial and annual expenditure of money. We have seen that German sanatoria have already cost more than a million for building and installation, and that their maintenance already costs a quarter of a million, and will soon cost three or four times as much. There is no reason for supposing that if we followed their example we should do so more economically.

It may, perhaps, facilitate the consideration of the subject if we assume, as I think we may, that the cost of erection of sanatoria might be met by grants from public bodies or by private benevolence. But it can scarcely be supposed that the current expenditure of these institutions could be supplied by voluntary contributions. Here, as in Germany, the sanatoria ought to be self-supporting; they must depend for their maintenance on the earnings of those benefited by them.

The German system of compulsory insurance against sickness is in reality a tax on labour, to the payment of which employers and employed contribute in something like equal proportion. Although, however, in principle there can be no more objection to the imposition of a tax on the earning of a workman who receives under £2 a week than to the tax which those whose incomes are not so very much larger submit to at the present moment with so little grumbling, it is not the less certain that in England any attempt to levy such a tax on the working man would not be tolerated. The most feasible way of accomplishing the end in view seems to be that the employers of labour should consent to levy upon themselves a contribution of which the amount would be proportional to and deductible in whole or in part from the wages paid to their workmen, on whom therefore their due share of the burden would ultimately fall. Such a system would be but a poor substitute for a compulsory system including earners of every class. Within its limited scope it would give equal or greater advantages to the insured, and would probably yield the means of maintaining a sufficient number of sanatoria for their reception when incapacitated by illness. But so far from being universal, it would exclude toilers of the poorer class—those who in Germany chiefly benefit from the operation of compulsory insurance. It would, moreover, fail as a means of promoting the timely removal of phthisical bread-earners from their homes and thus

preventing the spread of phthisis among the overcrowded population of our towns.

I have now come to the end of my task. I have placed before you the question of sanatoria for the sons and daughters of toil with all its difficulties. I have endeavoured neither to over-estimate its drawbacks nor to exaggerate its advantages.

The question is in the hands of the medical profession. In all matters which concern the bodily welfare of the people, our function is to furnish to those who are responsible for the government of the country guiding principles.

With reference to the present subject an experiment on a very large scale has been made for us in Germany. It is our duty to examine into the results, not for the purpose of imitating or adopting German methods, but with the view of making up our minds on a question of principle.

This question is whether on the one hand we should leave the consumptive bread-earner to struggle against his disease as best he may, contenting ourselves with promoting sanitary improvement and prophylactic measures against contagion, or on the other hand should, without in the least disregarding these objects or relaxing our efforts to prevent the spread of the disease, strive to organize a plan for enabling him to make the best of his life and earning power.

I venture to hope that this great question will be taken up by the approaching Congress and that the medical profession will be thereby awakened to its importance.

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